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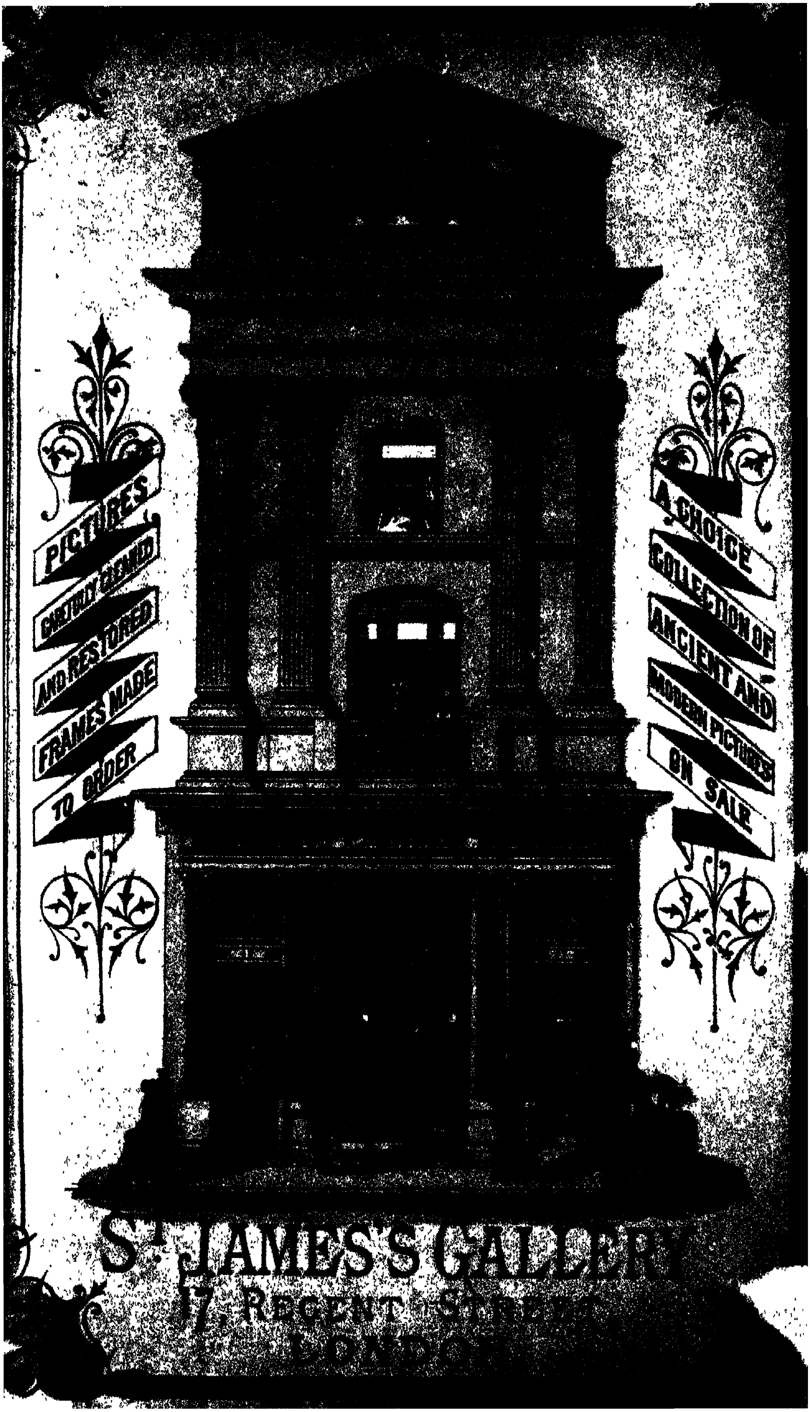
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POPULAR SCIENCE REVIEW.

THE HEAT OF THE MOON.

By J. CARPENTER, F.R.A.S.,
Of the Royal Observatory, Greenwich.

[PLATE LIV.]

OUT of the doubt that has so long shrouded the question of lunar caloreseence, a small gleam of certain light is now appearing, and meteorologists, however reticent they may have hitherto been upon the point, must henceforth, in justice, credit the moon with some influence, feeble though it be, upon the thermic conditions of our atmosphere.' Unless the lunar globe be composed of materials vastly different from those that form the earth—and we are not justified in supposing such a diversity—it must receive heat from the sun, and its surface must be raised in temperature as the earth's is: it must obviously be warmer than space; the heat it receives it must part with by reflexion and radiation, and we must receive our portion thereof. As the phase of illumination varies, as the sphere subjected to insolation turns towards us more or less of its heated surface, so, we may conclude, must the warmth shed upon us vary in degree; and it is inferable that greatest effect should be felt at the time of full moon, and the least at the period of new. This was the idea entertained by Toaldo, who was doubtless the first who tabulated thermometric observations having any pretensions to accuracy, with the view of detecting variations depending upon or related to lunar positions. He summed up a series of thermometer readings extending over forty years, dividing them into two sections—the one including fourteen days about full moon, the other, fourteen days about the epoch of new moon; and it resulted that the mean temperature of the full-moon semi-lunation exceeded that of the new-moon semi-lunation by about the twelfth part of a degree.*

* "Toaldo, Essai météorologique," etc. French edition. Chambéry, 1784, p. 77.

The difference thus shown is so small that it might be ascribed to accident, did not other and subsequent comparisons in a measure corroborate it. Buys Ballot tabulated the result of 118 years' observations, summing the temperatures for each day of the moon's age, and his result also showed a maximum of temperature at about the day of full moon, strictly speaking, a few days after.* But Mr. Park Harrison, who has classified several series of observations made in recent years, arrives at a conclusion almost the opposite of those of Toaldo and Buys Ballot. From readings of the thermometer at Greenwich, between 1841–1847 and 1856–1864, at Oxford between 1856–1864, and at Berlin between 1820–1855, he shows that the maximum occurs six or seven days after new moon and the minimum about four days after full. Mr. Harrison's curves laid down from his tabulation are remarkably consistent, and the difference between the maximum and minimum is very decidedly marked, for it amounts, from the average of all the series, to 2·5° Fahrenheit; that is to say, the whole heating effect of the moon upon Europe at first quarter is in excess of that at last quarter by two-and-a-half degrees.† Professor Loomis, using five years' observations made at Philadelphia for a similar investigation, arrived at a result strikingly coincident with this of Mr. Park Harrison's.‡ How are we to explain the discordance between Buys Ballot's conclusion and these? It may be that it is due to some slight difference in the grouping of the observations; or it may be that the periods covered by Harrison's and Loomis's groups are not sufficiently long to give a reliable result. Buys Ballot divided his 118 years into twelve-year periods, and if some of these had been taken separately, it is apparent from an inspection of his tables that they would have given evidence differing in some degree from that afforded by the whole number of years.

By either conclusion, however, the fact of lunar influence on temperature is pretty well established. Mr. Harrison's, it is true, appears paradoxical, for he makes the new and practically cold moon warm us, and the full and presumably hot moon cool us. His explanation, however, is philosophical: it involves a consideration that was put forth by Buys Ballot twenty years ago, to account in some measure for the small amount of lunar heat evidenced by thermometric investigations in proportion to that which we may suppose the full moon to shed earthwards. It is an accepted conclusion that the heat is of two qualities—

* Buys Ballot, "Changements périodiques de Température, dépendants de la Nature, du Soleil, et de la Lune." Utrecht, 1847, p. 79.

† "Monthly Notices of Roy. Astron. Soc.," vol. xxviii. p. 39.

‡ "Proceedings of American Association for the Advancement of Science," 1868.

dark or invisible, and luminous or visible—the former consisting of rays which have been absorbed by the lunar surface, and then radiated from it; the latter of rays coming as part and parcel of the reflected sunlight; and it is generally assumed that the former quality preponderates. Of course we can know nothing of the capacity of the lunar surface matter for receiving and radiating dark heat, but upon the premise that bad reflectors are good radiators, we may base a conjecture that this capacity is rather great, for the general surface of the moon can hardly be called a good reflecting one. Some of the craters and spots, it is true, shine under high illumination with what appears like metallic lustre; but these form an insignificant part of the whole area of the visible hemisphere, and between them and the general surface there is a vast difference of brightness. When we observe the moon telescopically, and screen the eye with a smoke-tinted glass, these bright spots glow like heated phosphorus, while the general surface of the moon presents the dusky appearance of brown paper. Assume the former to have even a metallic polish, and the reflective power of the latter comes out very low. If the one has less than this assumed lustre—and we can hardly expect the craters to be mirrors—the other must be dull indeed; and if so dull to the eye, it has doubtless a strong appetite for heat. Sir John Herschel inferred the temperature of the lunar crust, after its 300 hour-long day, to be far hotter than the boiling point of water on the earth.* The German physicist Althous from calculation deduced that it becomes heated to several hundred degrees of our thermometer's scale; † and Lord Rosse, as one of the results of his observations to be presently described, considered that the absorbed solar heat raised the moon's surface material to a temperature of about 500° Fahrenheit.‡

The invisible rays of heat being wholly or in part intercepted by transparent media, it is obvious that those of this quality which come from the moon do not reach the earth's surface, but are absorbed in the higher regions of the atmosphere. In this case they must be effective in evaporating high clouds, dispersing such as are light, raising and thinning those that are dense. Herschel ascribed to this influence the tendency of the full moon to clear the sky, which proverbs assert, and sailors and peasants believe in; and although an astronomical observer, Mr. Ellis of Greenwich,§ has refuted the grounds for such a belief, so far as ordinary cloud registers supply means of examination, it has been confirmed by Professor Piazzi Smyth's observations upon the high clouds over the Peak of Teneriffe.

* Herschel, "Outlines," sect. 432.
† Poggendorf, "Annalen," xc. 544.
‡ "Proc. of Roy. Soc.," xvii. 436.
§ "Phil. Mag." 4th ser. xxxiv. 61.

In the account of the famous experiments upon the mountain top made in 1856, this observer states that "on Guajura there appeared to be a strong tendency to upper clouds during several days preceding full moon, but on that night every particle of them disappeared: the lower clouds, however, were constant through the whole lunation. This does seem to confirm Sir John's idea; and to show, too, that the moon's heat, though effective at great heights, is entirely expended before arriving at the lower strata of cloud, 2,300 feet above the sea. The elevation of the upper clouds, which were apparently so effectually acted ön, we had no means of accurately judging of; but I should suspect that it could not have been less than 15,000 feet." *

Now, Buys Ballot pointed out, in 1847, that this absorption of heat by the higher airs is a cause of cooling to those atmospheric strata near the earth, since the evaporation of lofty clouds and the consequent clearing of the sky must permit a freer radiation of the earth's heat towards space, or at least into the higher regions of the atmosphere. And, as we have said, he considered this as a cause for the small amount of lunar warmth felt upon the earth. Mr. Park Harrison adopts this hypothesis to explain the high temperatures which he shows to occur at moon's first quarter, and the low ones which happen near the last quarter; and he considers that the time at which we observe the greatest effect in the former case is that when the part of the moon turned towards us has been least heated, and in the latter case that when the visible hemisphere has been longest subjected to insolation.

We might expect to see traces of the clearing effect of the warm moon in tabulations of cloud registers according to lunar periods; but we do not find them. Professor Schiaperelli has arranged thirty-eight years' cloud observations made at Vijevano in the order of days of lunation, and has laid down a series of curves which show the relations of clear and cloudy days to the moon's age.† But he shows the sky to be clearest when Mr. Harrison's theory requires that it should be cloudy, and cloudiest when, to fit the theory, it should be most clear. Meteorologists may one day reconcile these apparent anomalies. The evidence on either side is not, however, entitled to equal weight. Cloud observations have not the same accuracy as those of temperature: there is no instrument wherewith to make them. Much depends upon the judgment of the observer, and much also upon what is understood by "a cloudy night," or any other verbal record, and much again upon the interpretation of numbers denoting proportions of sky-area covered with cloud.

* "Phil. Trans.," 1858, p. 503.

† "Memoirs of the Lombards' Institute." 3rd ser. Vol. i. Fasc. iii.

Height and density of clouds are unheeded. Thermometer readings, on the other hand, are more definite, and less subject to erroneous classification; and we would rather accept their testimony of lunar influence on terrestrial temperatures, indirect though it be, than anything not susceptible of instrumental measurement. It would be a great point if we could measure the moon's dark heat directly, but this there seems little hope of doing, unless, as Professor Smyth suggests, we ascend to the level of the high clouds which he saw evaporated by the full moon—an altitude of about three miles.

The visible heat rays, and those which are not intercepted by the atmosphere, we can now, thanks to modern means of thermometry, measure with some pretensions to accuracy. Whether the moon's light possesses perceptible warmth has been a point of anxious and tentative inquiry among astronomers and physicists for a century and a half at least. The first observer who recorded a trial of it was, I believe, Geminiano Montanari, who, in 1685, thought he had indications of lunar heat in an ordinary dilatation thermometer. The second was Tschirnausen, the famous burning-glass maker. He published, in the year 1699, an account of a wonderful double lens, which concentrated the solar rays so far as to make them melt and fuse metals. It was formed of a four-foot burning-glass, with another lens of smaller size behind it. In telling of a number of its achievements, he mentions, little more than incidentally, that he turned it upon the full moon, but, though the image formed in the focus was of great brilliancy, there was no sensible heat.* He does not say what thermometer he used. Six years after, in 1705, La Hire the younger made an experiment with a burning mirror, belonging to the Paris Observatory, of 35 inches diameter, and an air and mercury thermometer of the construction then recently proposed by Amontons. The bulb of this instrument was two inches in diameter, and when it was placed at the focus of the mirror the moon's reflected image just covered it. The height of the mercury was noted, and the condensed moonlight was kept upon the bulb for a considerable time, but there was no alteration in the reading. This one trial satisfied La Hire that the moon's light was heatless.†

We find no record of attempts at lunar thermometry during the hundred years following the date of that of La Hire. But in 1820 attention was again turned to the subject by Professor Howard, of the Maryland University in the United States. Thermometers had by this time improved in construction: the instrument used by Howard was a modification of the differential one proposed by Leslie. The condenser was a mirror thirteen

* "Histoire de l'Académie," 1699, p. 90. † Ibid., 1705, p. 346.

inches in diameter, and one bulb of the thermometer, previously blackened to increase its heat-absorbing power, was mounted at the focus. The reflector was opposed to the light of the full moon, and, to quote the observer's words, "the liquid began immediately to sink, and in half a minute was depressed eight degrees, when it became stationary. On placing a screen between the mirror and the moon, it rose again to the first level, and was again depressed on removing this obstacle. I repeated this experiment several times to satisfy myself, and some of my friends who happened to be present, that there was no fallacy in the conclusion of its being a positive proof of the calorific power of the lunar rays, and at the same time affording an evidence of the great delicacy of the instrument."* The eight degrees here indicated were not those of any recognised thermometric scale, but mere arbitrary divisions upon the tube, about a millimètre apart. Within a year or two Pictet repeated Howard's experiment, using a similar thermoscope, but his index remained unmoved under lunar influence; if it altered at all, it gave an indication of cold. Prevost, reporting these results, pointed out that a mirror apparently reflects cold when exposed to the clear sky, because it intercepts the earth's warmth, and leaves the thermometer free to radiate its heat towards the sky. He thought the same effects would be produced whether the reflector be turned to the moon or to any other part of the heavens; and he suggested the desirability of experimenters making this test a part of their lunar thermoscopic observations. Further, he remarked that heat might be experienced occasionally, on fine summer nights for instance, for then the upper air is warmer than the lower.† Howard's strongly manifested warmth may have come from this source; possibly Montanari's also, and some detected by Frisius in 1781. Professor Volpicelli, in some historical notes on lunar thermometry, mentions certain researches by a Mr. Watt, which seemed to confirm Howard's experience. This was Mr. Mark Watt, a member of the Wernerian Society. His experiments, besides being rather unphilosophical, hardly bear upon the point: they refer rather to some supposed attractive and repulsive actions exercised by the moon's light upon little discs of metal mounted at the ends of a balanced bar.‡ There is mention of heat from the lunar rays, but it is ambiguous.

The experiments so far prosecuted, while they evidenced the anxiety of physicists to settle the point at issue, only proved the inefficacy of the means which, in expansion thermometers, they possessed for the purpose. But presently came a revolu-

* "Silliman's Journal," vol. ii. p. 329.

† "Bibliothèque universelle," xix. 35.

‡ "Edinburgh New Philosophical Journal," xix. 122.

tion in the science of heat measurement: Seebeck, in 1822, discovered the power of thermal currents to excite electricity in metallic conductors; and Nobili adapted the discovery to the construction of a thermoscope which permitted the making of experiments that would have been declared impossible a very short time before. The years about 1830 found the inventor of the thermopile, in conjunction with Melloni, engaged upon investigations involving the measurement of temperatures separated by very small fractions of a degree upon any existing scale. The passage of heat through transparent bodies, the bodily warmth of insects, the calorific exhalations accompanying the luminous glow of phosphorus—these were the subjects upon which were first tested the powers of the new instrument, which was so delicate that it felt the warmth of a human body thirty feet removed from it. Having refuted the common opinion that luminous phosphorus exhibited the phenomenon of light without heat, Melloni sought to disprove the similar idea sometimes entertained with regard to the light of the moon. The pile he employed was formed of thirty-eight pairs of bismuth and antimony bars, soldered together at their alternate ends, and packed closely, though electrically isolated, into a metallic hoop, the first and last bars being connected by wires with the coil of a delicate galvanometer. A conical reflector surrounded the exposed ends of the bundle of bars which formed the "face" of the pile; this face was coated with lamp-black: the lunar rays were concentrated upon it by a concave metallic mirror, the diameter of which Melloni does not mention; but whatever effect they might have produced was completely shrouded by that of the cold received from the sky, or, to speak more correctly, the heat escaping by radiation from the exposed surface of the pile.* It must be borne in mind that currents are developed in the bundle of plates or bars whenever the equilibrium of temperature between the two faces is disturbed *in any way*. If one face is boxed up and the other is opened to the view of a clear sky, the latter will cool more rapidly than the former, and the effect will be as if the covered face had been actually warmed. Melloni, in his lunar experiments, found his galvanometer always pushed to its limit of divergence from this cause; and for a time he was frustrated. Several years after, adopting precautions to neutralise the frigorific effect of the clear sky, he was able to obtain decided heat indications, as we shall presently see. In the meantime, Professor Forbes, taking advantage of the delicacy and prompt action of a pile he had constructed for some experiments upon the refraction and polarisation of heat, renewed Melloni's experiment, using a lens

* "Annales de Chimie," xlviii. 211.

instead of a mirror, and thereby preventing the escape of the pile's heat towards the sky. The lens in question was of the polyzonal form, having a diameter of thirty inches and a focal length of forty-one inches; giving an image of the full moon 0·38 inch in diameter, and thus securing a concentration of its light and heat to the extent of 6,000 times. A high full moon in December 1834 furnished good conditions for a trial: the pile was mounted in the focus of the lens, and the moon's rays were alternately thrown upon and screened from it some twenty times during the hour and a quarter that the experiments lasted. There was an occasional movement of the needle amounting to not more than a quarter of a degree, but, as Forbes pointed out, the greater part of this was due to the dynamical effect of an instantaneous impulse. To form an estimate of the amount of heat which this deflection represented, the number of degrees upon the galvanometer scale equivalent to a centigrade degree was ascertained by exposing the pile and a thermometer to one and the same source of heat. Two thermometers were, in fact, used for this comparison; and the mean value of a degree's deflection of the needle was found to be one-fiftieth of a centigrade degree. Supposing that the statical effect of the moon-heat upon the pile-needle amounted to one-eighth of a degree, we have evidence of heat to the extent of $\frac{1}{400}$th of a degree centigrade. But this was the measure of the condensed heat: assuming that the lens, in consequence of dispersion, reflection, and absorption, concentrated the light 3,000 instead of 6,000 times, and making a further allowance for the proportion of the pile-face covered by the moon's image, Forbes concluded it as improbable that the direct light of the moon would raise a thermometer one-three-hundred-thousandth part, or, decimally written, 0·0000033 of a centigrade degree.* Judging from later results, it seems probable that the galvanometer needle employed by Forbes was not sufficiently delicate to answer to the action of the current furnished by the pile.

Melloni's new experiments were made in 1846 with a polyzonal lens of a mètre diameter, constructed for the Meteorological Observatory at Vesuvius. At first he was plagued with the abstraction of heat from his pile, as he had been before when using a reflector; but he got over his difficulty by mounting the lens and pile within doors, allowing the moonlight to enter through an open window. When all was in readiness for observation, and the rays were first cast upon the instrument, the needle moved several degrees in the direction indicating heat; but upon covering and re-exposing the face of the pile,

"Transactions of Edinburgh Roy. Soc.," xiii. 139.

much to Melloni's surprise, it turned towards cold. He traced the cause of this to draughts of cool air impinging upon the thermoscope. These could have been remedied by ordinary means; but the more philosophical cure was to cover the pile with plates of glass, which would allow heat to pass through to it but yet keep all air-tight. Two screens were then interposed, and upon the next favourable occasion the experiments were renewed. This time they were perfectly successful: the needle, stationary when the pile was uncovered, soon began to move slowly to the heat-side of the scale, and at the end of five minutes' exposure it reached its maximum of deflection, 3·7 degrees of arc. The pile was covered, and it returned to zero; uncovered, and it turned heat-wise. This alternation was repeated many times, and always with the same qualitative result. Subsequently, trials were made in the presence of MM. Belli, Mossotti, and Lavagna, and many other distinguished *savans*, all of whom went from the chamber, says Melloni, convinced that the light of the moon is calorific.* No detailed observations are given by Melloni, but he says that he assured himself that the lunar heat varies, as one might suppose it would, with the age of the moon and with its height above the horizon: he intended to determine the actual amount of the heating effect, but it does not appear that he ever did so. Prof. Zantedeschi confirmed Melloni's conclusions in 1848 by observations made with a mirror of 0·6 mètre diameter, and a pile made by Gourjon of Paris, used upon the clear full moons that shone in the summer sky of Venice.

Favourable circumstances offered for catching some of the heat-rays that are ordinarily absorbed by the lower atmosphere, when Professor Piazzi Smyth, in 1856, ascended Teneriffe to test the supposed advantages of an elevated station for delicate astronomical observation. A thermopile accordingly formed part of the equipment of the expedition, but no burning lens or mirror was taken to be used with it. The pile was simply furnished with a polished metal cone of rather larger base than the area of the face, and the experiments were made by alternately turning the cone towards the moon and to a part of the sky 20 degrees east or west of it. Two nights in August 1856, that of the full moon and the following one, afforded favourable opportunities for observation, though the luminary was rather low, having an altitude of about 45 degrees. The effects of heat were unmistakable: on each night some thirty readings of the galvanometer were taken with the pile alternately on and off the moon; and the mean deflection in the direction indicating warmth derived from all the

* "Comptes rendus," xxii. 543.

observations, which throughout gave consistent results, was 0·37 of a degree. To compare this effect with that from a terrestrial source of heat, Prof. Smyth placed a Palmer's candle 14 ft. 9 in. from the pile, and took readings of the needle with the same alternations of exposure as he had adopted for the moon. Twenty-one readings gave a mean deflection of 0·77 degrees; so that the deduced heating influence of the moon appeared equal to half that shed by a candle to a distance of fifteen feet.

Professor Smyth did not ascertain what this would be in terms of a recognised scale; but M. Marié-Davy has since done so, using, however, not the mean of the two nights' observations, but that given by the first night's, and assuming that the heating power of the candle used by him was not sensibly different from that employed on Teneriffe. The first or full-moon night's deflection was 0·25 of a degree, or a third of the candle-effect. Marié-Davy's candle, at fifteen feet from his pile, gave a deflection to his needle amounting to 17·3 divisions. A third of this would be 5 8 divisions, and this (the scale value of his needle being known to M. Marié-Davy) answered to 0·00075 of a degree centigrade for the heating effect of the moon upon a body on the summit of Teneriffe.

Passing over some negative results arrived at by Prof. Tyndall, in 1861,* from trials made upon the roof of the Royal Institution, without a condensing means beyond a large cone, and which were explained upon the hypothesis put forth by Buys Ballot, we next come to the experiments of the Earl of Rosse, for the making of which far superior means were available than any at the disposal of previous investigators. One of the famous Parsonstown reflectors — the "three-foot"—was equipped specially for the work. The difficulty of compensating for the effects of radiation on the anterior face of the pile compelled the adoption of two piles,† which were connected "in such a manner that a given amount of heat on the anterior face of one pile produced a deviation equal in amount and opposite in direction to that produced by an equal amount of heat on the anterior face of the other pile." Small concave mirrors were used to still further concentrate the lunar rays collected at the focus of the great mirror. The galvanometer was of the construction introduced by Prof. Thompson to meet the requirements of the delicate currents employed in sub-Atlantic telegraphy. Instead of the needle pointing to engraved degrees upon a circle beneath it, a mirror is attached to the suspending bar, and a lamp-flame is reflected from this upon a distant scale. In this manner, deflections imperceptible

"Phil. Mag." 4th ser. xxii. 377, and 470.
See Plate and description.

in the needle itself can be magnified to any extent by increasing the distance between the lamp and the scale. This form of instrument has been used by Mr. Stone for the purpose of measuring the extremely feeble heat radiated from the stars: it is possible that but for its capabilities these delicate experiments would never have been attempted.

Lord Rosse essayed to determine the proportions of moon-heat coming from three sources: 1. That which it may be supposed the moon sheds in virtue of its being a globe possessing internal or cosmical heat. 2. That solar heat which is reflected from the surface with the reflected light. 3. That which, coming from the sun, is first absorbed at the lunar surface and then radiated as dark heat. The first and third qualities are such as we should expect to find but slightly manifested at the earth's surface by reason of the intercepting influence of the atmosphere. The second source is that from which we might expect the most marked results. Observations were made during the winter and spring of 1868–69, on sixteen nights in all, on some of which the moon was several days from full. At five days old it gave evidence of heating power; and the amount of deviation of the needle increased with the phase, showing that the law of variation of the moon's heat does not differ much from that of the moon's light. The maximum of one was found to be coincident in time with the maximum of the other; there was no evidence of greatest heat coming at the period of greatest insolation, which would be at about third quarter.

To test the proportion of light to dark heat in the rays reaching the mirror, the Earl of Rosse placed screens of plate glass before the pile on four nights near the time of full moon, and the result was a great interception of heat, manifested by a small deviation of the needle. About 8 per cent. was considered to be the probable amount that passed through the screens, from which Lord Rosse concludes that the greater part of the lunar heat is that which has first been absorbed by the moon's surface and then radiated from it.*

By computation in one case, and by observation of the relation of solar and lunar heat to that of a vessel of heated water in another case, the relation of solar to lunar radiation was found to be nearly as 80,000 to 1. And by the use of a blackened cistern of hot water, subtending the same angle at

* This absorbed and radiated heat is presumably dark. How then did it get through the atmosphere? There would seem to be heat which passes through air, and yet is obstructed by glass. One result of thermoscopic investigations upon the light of moon and stars may be to improve our knowledge upon such points as this.

the small concave reflectors as the large mirror of the telescope, Lord Rosse found that such a deviation of his galvanometer as the full moon gave appeared to indicate an elevation of temperature of the lunar surface, as we have previously mentioned, through 500° Fahrenheit, allowance being made for the imperfect absorption of the sun's rays by the lunar surface.

No evidence was afforded of cosmical heat. Had there been such, and it could have penetrated our atmosphere, a certain heat-effect would have been constantly exhibited, independently of the phase of illumination. On one night, however, when the moon was two days old, and the air clear and calm, no perceptible impulse was imparted to the needle. It is reasonable to suppose that, under favourable circumstances for revealing it, some internal warmth would show itself, small though its amount might be; for although the moon, from its large radiating surface and small mass, doubtless parted rapidly with the heat of the previous existence of which we see evidence in its volcanic crust, yet it can hardly have cooled down to the low temperature of space, and if it has not, the dark body of the moon ought to exhibit warmth not manifested by the regions of sky surrounding it. It is conceivable, however, that any such that might come towards us would be absorbed in the higher strata of our atmosphere.

Since Lord Rosse's observations were made, renewed attention has been given to the subject by physicists on the continent. During the past summer, M. Baille, of the Paris École Polytechnique, verified preceding conclusions as to the evidence of lunar radiation; and, by an experiment similar to one of Lord Rosse's, determined that the full moon gave as much heat to his pile as a cube of boiling water 6½ centimètres square at a distance of 35 mètres. A Thompson's galvanometer was employed, and the rays were condensed by a concave mirror of 39 centimètres' aperture.* M. Marié-Davy, after fruitlessly trying to affect a dilatation thermometer by the moon's rays, condensed by a three-foot lens, mounted an extremely sensitive thermopile in the eyepiece of an equatorial telescope of nine inches' aperture, erected in the garden of the Paris Observatory. A little trouble was caused by the influence of some iron ribs of the dome covering the instrument, but the effects of these were avoided by comparing each deviation of the galvanometer needle with the mean of the two readings before and after it. Observations were made on five days between the 9th and 20th of October last, the fourth and fifteenth days of the moon's age. Like Lord Rosse, M. Marié-Davy obtained effects increasing in magnitude with the degree of illumination; and knowing

* "Comptes rendus," lxix. 960.

the value of his galvanometer scale in terms of the centigrade, he has been able to give the actual observed warmth upon each occasion of observation. The following is his table of results:

Date 1869	Age of Moon	Mean Solar Time (Paris)	Mean Deviation of Needle	Value in parts of Centigrade Degree
	days	h. m.	d.	°
Oct. 9	4	7·32	1·3	0·00017
„ 10	5	7·46	1·0	0·00013
„ 12	7	8·45	5·8	0·00075
„ 12	7	9·12	2·2	0·00029
„ 17	12	8·39	20·0	0·00260
„ 20	15	10·11	22·1	0·00287

The second series of observations on the 12th were made with the moon approaching a foggy horizon: the result shows the enfeebling influence of a low altitude or a misty atmosphere. The object-glass here used concentrated the light 247 times, so that the deviation on the full-moon night, ascribable to direct influence, corresponds to twelve-millionths of a degree.*

Collecting the comparable results, we have for the following determinations of the directly measurable lunar warmth in decimal parts of a centigrade degree—

Forbes, less than	0·0000033
Smyth (on Teneriffe)	0·00075
Marié-Davy	0·000012

These are perhaps as consistent as we can expect such difficultly determinable figures to be. They agree in showing the available lunar heat at the earth's surface to be a quantity only expressible by a number in the fourth or fifth decimal subdivision of a degree centigrade. Such a quantity can have no effect upon terrestrial temperatures: whatever traces of lunar influence are shown upon thermometer registers, and we have seen that such are strongly manifested, must be those of dark heat acting indirectly upon our lower atmosphere.

But the subject is by no means exhausted. What Lord Rosse has done he regards as initiatory, and what Marié-Davy has done he on his part puts forth as merely an instalment of results at which he hopes to arrive by isolating and evaluating if possible the dark as well as the luminous calorific rays of the moon. This paper, it is hoped, having brought up the subject to the present time, may prepare interested minds for whatever may follow.

P.S.—Since the foregoing was written, M. Marié-Davy has given to the French Academy the results of a new series of observations made in November with a mirror of eight inches

* "Comptes rendus," lxix. 922.

diameter instead of a lens, and with a modification of the double pile arrangement used by Lord Rosse. Only one pile was really used, but it was so mounted that both faces were similarly exposed to atmospheric influences. The results confirm Lord Rosse's *conclusions that the moon has no cosmical or internal heat to give us; that the heat she does impart varies as the phase of* illumination; and that the lunar is to the solar radiation as 1 to 80,000. Further than these, Marié-Davy infers that the diffusive power of the moon's surface is about the same as that of chromate of lead, as determined by MM. Desains and Provostaye, and that the moon's heat, by reason of its large proportion of obscure rays, is much more impressionable by atmospheric humidity than solar heat. He found that, with the moon at the same age, his measured heat in November was six times as great as in October. He ascribes this excess to the use of a mirror instead of a lens, and concludes that the latter absorbs six times as much lunar caloric as the former.†

"Comptes rendus," lxix. p. 1154.

DESCRIPTION OF PLATE LIV.

A A. Converging beam from great reflector.

B B. Small concave mirrors (fixed upon a bar screwed to the eye end of the telescope), one of them receiving the beam and condensing it upon the corresponding pile.

C C. The two piles, each composed of four pairs of bismuth and antimony plates; the faces exposed to the mirrors being laterally sheltered by metal cones, and their posterior faces covered with brass caps filled with water for maintenance of nearly uniform temperature.

D D. Wires leading to the galvanometer.

E. Reflecting galvanometer formed of *a*, a coil of fine wire surrounding *b*, a delicate magnetised needle covered by a small plane mirror, suspended by a filament of silk, and rendered partially astatic by the second small magnet *c*, rigidly connected with it. *d* is a large arched magnet sliding upon a vertical bar; its use is to adjust the zero of the needle and regulate its sensitiveness.

F. Lamp. A beam from its flame passes through a hole in the board G and falls upon the mirror at *b*, thence being reflected on to a graduated scale on the opposite side of G. The displacement of the light-spot is a greatly magnified measure of the needle's deflection.

UNDER CHLOROFORM.

By B. W. RICHARDSON, M.D., F.R.S.

I HAVE taken as the text of one or two papers, the familiar title given above, not with the strict intention of confining what I have to say to the one subject, chloroform. But, accepting chloroform as a type of a number of chemical substances which possess the property of inducing sleep and insensibility, I propose rather to consider the general subject of sleep and insensibility as induced by artificial means. It will be my object to write so simple an account of the progress of discovery on the subject in hand, that the intelligent reader may easily follow me and may, in the end, feel himself in safe possession of all the more important truths which modern science has brought to light in relation to anæsthesia.

Historical Notes.

The practice of destroying, or rather of suspending consciousness, for and during the performance of surgical operations, although it has been developed in our time, is as old as the practice of administering soothing remedies for the relief of pain in the course of disease. The reference to mandragora so often made in classical works of old, and in some of our earliest English poets, bears on the employment of an agent of this kind. Mandragora, or mandrake, belongs to the natural order of plants *Solanaceæ*, and is of the same genus as the *Atropa belladonna*, deadly nightshade. The *Atropa mandragora* is a plant common to the Isles of Greece, and was used by the early Greek physicians as a sedative in various ways. The plant is said to be more determinate in its action than *belladonna*, and the odour is much more unpleasant. Very soon after the introduction of ether as an anæsthetic, Sir James Simpson pointed out that the mandrake had been recommended by Dioscorides for the specific purpose of making surgical art painless, and he quoted the passages in which the plan is described. "Some persons," says Dioscorides, "boil the

root of mandrake in wine down to a third part, and preserve the decoction, of which they administer a cyathus, about a fluid ounce and a half, in want of sleep and severe pains of any part, *and also before operations with the knife, or the actual cautery, that they may not be felt.*" Speaking further on of a similar decoction which is diluted with wine, he says, "*three cyathi of this wine are given to those who require to be cut or cauterised, when, being thrown into a deep sleep, they do not feel any pain.*"

The same author, Dioscorides, describes a kind of mandragora called morion; he records, that a drachm of it being taken as a draught, or eaten in a cake or other food, causes infatuation, and takes away the use of the reason. The person sleeps without sense, in the attitude in which he ate it, for three or four hours afterwards. Medical men also use it when they have to resort to cutting or burning.

Pliny, after Dioscorides, is more circumstantial still in respect to mandrake, teaching that the leaves are more potent than the root. The draught of its preparations may, he says, kill, and it has the power of causing sleep in those who take it. The dose is half a cyathus. It is taken against serpents and before cuttings and punctures, lest they be felt. And then he adds, very curiously in respect to the medicine, "for these purposes *it is sufficient for some persons to have sought sleep from the smell.*" And yet another author, Apuleius, speaking of mandragora, says of it, "If any one eat of it he will immediately die, unless he be treated with butter and honey, and vomit quickly. More, if any one is to have a limb mutilated, burnt, or sawn, he may drink half an ounce with wine, and while he sleeps the member may be cut off without any pain or sense."

I have followed, in relating these details, the very clear, able, and concise work of the late Dr. Snow.* The reader will probably agree with an observation he makes on the strangeness of the fact, that with such passages from such well-known authors in existence, the practice of preventing pain during surgical operations was entirely unknown just prior to the year 1846.

The fact is still more singular, when we find that through the middle ages, and through the period of the revival of letters, and down to the last century, notions of the practice of annulling pain before operations, and even experiences of the practice, were constantly cropping up. Snow, in the chapter of the work already cited, gives us several more instances of this kind. He quotes from the work entitled Koukin-i-tong, a general collection of ancient and modern medicines of the Chinese, that one Hoatho, a physician, who lived about the

* See "Snow on Chloroform and other Anæsthetics." Edited by Richardson. Churchill and Sons, 1858. Historical Introduction, pp. 1–36.

years 220–230 of our era, gave to his patients, when he had to make a deep incision or moxa, a preparation of hemp (Ma-yo), and at the end of some moments he, the patient, became as insensible as if he had been drunk or deprived of life. Then the operation was performed without pain. From the rapidity with which the insensibility by this author is said to have been produced, Snow is of opinion, that the fumes of the hemp must have been inhaled while the substance was being burned; and in support of this view, he quotes that the ancient Scythians, according to Herodotus, were in the habit of inhaling the fumes of hemp until they produced drunkenness. This hypothesis is in accordance with other experience, the practice being an old one to administer the fumes or smoke of various burning substances by inhalation. The application of the ammoniacal fumes of the burning feather is a case directly in point, and another very ancient practice in the treatment of bees is equally significant. For many ages it has been common in this country, and in various parts of the Continent, to rob the beehive of its rich contents without destroying the industrious occupants of the hive, by subjecting the bees to the fumes of the fungus, called in common language "puff ball" and in technical language "*Lycoperdon giganteum.*" Some years since, I think about 1851, I investigated this practice, and found that the fumes of the *Lycoperdon* supplied a volatile substance, which produced the most pe ːct insensibility. I performed many veterinary operations ｒ various animals while they were under the influence of this ꜱleep-producing agent, and I suggested its general application for painless operations on the lower animals. Afterwards Dr. Thornton Herapath, a young chemist whose career, most promising, was cut short by death, followed up my enquiries, and on making a more perfect analysis of the fumes, discovered that the narcotic agent was carbonic oxide. A third illustration of the hypothesis of Snow is afforded, again, in the plan of inhaling the fumes of burning opium, until the production of intoxication. In the intoxication after smoking of opium all sense of pain is annihilated; and as the practice is attended with very little immediate danger to life, it is most curious that in the East it was not adopted, generally, preparatory to the performances of the surgeon. Opium used in this way would answer well; for I remember, when I was experimenting with the fumes of the burning *Lycoperdon*, inhaling them, and comparing their effects with the effects produced by the smoke from a little opium in a pipe, there was very little difference of effect; and I found that, even in the early stage of stupor from opium, the skin was insensible to pain.

The hypothesis that a volatile narcotic was administered by inhalation by the Chinese, is yet more clearly supported by the

fact, that the inhalation of the smoke of haschisch has been a long-prevailing custom in the East. This is the same substance as the *nepenthes* of Homer, and the preparation of it as a luxury forms a distinct trade or calling. The distinguished Polli of Milan, who has recently investigated the action of haschisch, explains that the substance is sold in candle-shaped pieces, from three to four inches long: it smells like impure wax, is sharp to the taste, is easily soluble in water, and is taken either by the mouth in honey or coffee, or by the process of smoking. Occasionally, it is taken in the form of a spirituous liquid or wine. In all cases, the active principle of the substance is derived from the flowering extremities of the hemp, the substance said to have been used by Hoatho; the active principle resides in a resin peculiar to the hemp-flower of the East; this variety of hemp is called the *Cannabis Indica*, and the resin is called *cannabina* or *haschischina*.

When the haschisch is smoked or swallowed, or when the simple flower and seed are smoked or swallowed, there is induced a peculiar inebriation, which follows rapidly under the inhaling process, slowly after the eating, and lasts, in each case, a considerable time. Polli subjected himself and two friends to the action of the hemp, and describes the effects with singular clearness and power. The most noticeable coincidence is the similarity of the symptoms brought out with those which are produced by the inhalation of nitrous oxide gas mixed with air. In fact, Sir Humphrey Davy's description of his sensations under the influence of the gas, and Polli's description of his sensations under the influence of haschisch are so alike, it could be inferred that the same agent was at work. The haschisch not only modifies ideas and conceptions of surrounding objects, and excites new ideas respecting time and space, but it removes common sensibility, so that a blow may be administered to a person under it without exciting either pain or anger.

Snow notices yet another author who recommends a volatile anæsthetic. This author is the famous Theodoric of the thirteenth century, who gives a recipe after Dominus Hugo. Dominus directs that "there be taken, of opium, of the juice of the unripe mulberry, of henbane, of the juice of hemlock, of the juice of the leaves of mandragora, of the juice of the woody ivy, of the juice of the forest mulberry, of the seeds of lettuce, of the seeds of the dock which has large round apples, and of the water hemlock, of each an ounce. These are all to be mixed in a brazen vessel and then in the vessel is to be placed a sponge: the whole is to boil so long as the sun lasts in the dog days, until the sponge consumes all, and all is boiled away in the sponge. The sponge is now to be kept, and as oft as there is need of it, it is to be placed in hot water for an hour and afterwards applied

to the nostrils of him who is to be operated on until he has fallen asleep, and so let the surgery be performed." The success of this method of applying a sleep-producing sponge, as detailed above, is disputed by Snow, on grounds which seem sufficient, theoretically speaking. He did not, however, put the question practically to proof, and it is possible, I think, that a sponge prepared in the manner described might contain in its porous substance many bodies which could be volatilised by heat, and which would, if inhaled, produce sleep. If the instruction had been that the sponge, or portions of the sponge were to be burned, and the fumes of the burning substance inhaled, the effectiveness of the procedure would have been sufficiently clear.

Whatever had been, in their day, the value of any of these plans, they died out, and failed altogether to become systematised. One mode, the offshoot probably of the mandrake wine system, was occasionally followed, and that consisted in administering, by the mouth, a dose of opium previous to operation. It was probably a dose of this drug which was administered to Augustus, king of Poland and Elector of Saxony, as told in Meissner's *Skizzen*, quoted by Dr. Silvester, *sic* :—

"Augustus, King of Poland and Elector of Saxony, suffered from a wound in his foot, which threatened to mortify. The court medical men were opposed to the operation of amputation; but during sleep, induced by a certain potion surreptitiously administered, his favourite surgeon, Weiss, a pupil of Petit, of Paris, cut off the decaying parts. The royal patient was disturbed by the proceeding, and inquired what was being done, but on receiving a soothing answer he again fell asleep, and did not discover till the following morning, after his usual examination, that the operation of amputation had really been performed."

Bell, in his *System of Surgery*, published in Edinburgh last century, speaks of the practice of administering a large dose of opium prior to surgical operations; but Bell was not favourably impressed with the practice, because it produced vomiting; and his opinion would appear to have been pretty generally shared.

The history of the systems of removing pain by causing general insensibility of body and unconsciousness, is, I think, fairly exhausted in the above recitals, that is, up to our own time. But it must not be omitted, that various local methods for removing pain were also devised of old: some of these are deserving of remark. I notice again, after Snow, a receipt of Theodoric from Master Hugo for this purpose. Antimony, quicksilver, soap, quick-lime, and a little arsenic are to be sublimed together, says the Dominus Hugo, and a portion of

the resulting compound, the size of a nut, is to be applied over the part that is to be operated on. The arsenic so sublimed is described, adds Snow, "as rendering surgical operations extremely pleasant;" but with his keen scepticism he analyses, distrusts, and practically does away with the whole story.

The benumbing influence of extreme cold may be accepted as a natural discovery, coeval with the existence of mankind in the temperate and frigid zones, and physicians, at an early date, seemed to have used cold for the relief of pain. Its systematic use probably came in later; after the revival of letters, so called. It is related of Harvey, the discoverer of the circulation of the blood, that he was accustomed to seek relief from the pain of gout by going on the leads of his house, and there immersing his painful foot in ice-cold water. But we have to name another man, living in the same age, to see the same remedy employed in a direct manner in order to remove sensation in a part of the living body before subjecting the part to operation. This man was Thomas Bartholinus, one of the most learned and industrious masters in physic. Bartholinus wrote a treatise of 232 pages on the medical uses of snow ("De Nivis Usu Medico"). The book, from beginning to end, is wonderfully suggestive, and in the twenty-second chapter he conveys the practice of applying extreme cold to produce insensibility before the performance of surgical operations. The plan, he says, was taught to him by Marcus Aurelius Severinus of Naples. In our day the same application of cold has been, independently, brought into use by a man of singular originality and genius, Dr. James Arnott.

One more method calls for a word, and that is the method of removing sensibility to pain by means of pressure. This plan is fully described by Bell, in a chapter entitled, "Preventing or Diminishing Pain during Chirurgical Operations." He says it has long been known that the sensibility of any part may not only be lessened, but even altogether suspended, by compressing the nerves which supply the part. To effect this systematically, Mr. James Moore, of London, invented, in 1784, an instrument which produced pressure on the large nerves, an instrument which was tested by no less a surgeon than John Hunter, in St. George's Hospital. It partially succeeded, but the pressure had to be maintained an hour before the operation, and the proceeding fell into disuse.

We are brought now to the early part of the present century, and to the researches of Sir Humphrey Davy. Sir Humphrey discovered that nitrous oxide gas, when inhaled, produces insensibility to pain, a kind of intoxication, and sleep. In the course of his enquiries he tells us that he had a very good opportunity of ascertaining the power of the immediate operation of the gas in removing intense physical pain. In cutting one of

the unlucky teeth called *dentes sapientiæ*, he experienced an extensive inflammation of the gum, accompanied with great pain, which equally destroyed the power of repose and of consistent action. On the day when the inflammation was the most troublesome, he breathed three large doses of nitrous oxide. The pain always diminished after the first four or five inspirations, the thrilling came on as usual, and uneasiness was for a few minutes swallowed up in pleasure. As the former state of mind, however, returned, the state of organ returned with it; and he once imagined that the pain was more severe after the experiment than before. The results of Sir Humphrey Davy's experiments led him to suggest that—"*as nitrous oxide in its extensive operation appears capable of destroying physical pain, it may probably be used with advantage during surgical operations, in which no great effusion of blood takes place.*"

In these observations of Sir Humphrey, a new era was opened. Nearly forty years truly elapsed before the idea above suggested received practical form and accomplishment, but inhalation of nitrous oxide gas was never lost sight of; year by year it was used, in play, in the chemical schools, and many a student, laughing under the inebriation it produced, was pinched and thumped unconsciously by his fellows—operated upon *in fun.*

Meanwhile, comparisons were being made between the action of nitrous oxide and some other agents, the most important fact elicited in this direction being, that the vapour of sulphuric ether possesses an analogy of action. We touch in these words the history of the modern development of the science of anæsthesia. Two men claim here to have been first pioneers; these are Dr. Collyer and Dr. Horace Wells, both Americans. Collyer, whose name has hardly been heard of in the matter, adduces evidence that he performed a capital operation under anæsthetic sleep, as early as the year 1839. His evidence is, in my opinion, strong, but I have not seen the original document, which, I am told, renders it conclusive. Wells's claim dates from December 11th, 1844, when he inhaled nitrous oxide at his own request, from a Mr. Colston, while Dr. Riggs, a dentist, extracted for him a tooth. Two years later Dr. Morton, also an American, began to perform operations under sulphuric ether.

At this point our history shall stop: it is too closely contemporaneous to be written at length, without the possibility of injuring unintentionally some fame, or touching some susceptibility, or exciting some mental pain. Suffice it to say, that the work which has since been done by all, has been unequalled in its time in science, unequalled, I mean, in industry, earnestness of purpose, and usefulness to the whole human race.

Modern Agents for Anæsthesia.

The original chemical bodies, nitrous oxide gas and sulphuric ether, have, notwithstanding much opposition and many advancements of rivals, retained a certain fair position as anæsthetics. Nitrous oxide, indeed, has of late been greatly extolled and raised into fervour of favour by the members of the Dental profession. Ether has been persistently employed in some parts of America and some parts of France, and its friends have quite recently claimed for it that it has not been the cause of one single death.

It is right that the members of the public, who, in common parlance, know only that sometimes they or theirs may have to be put "under chloroform," should know these and all other facts correctly, and intelligently; should know what agents are used and the relative value of each agent in respect to practicability of administration, certainty of action, and freedom from risk. I will essay, therefore, to bring these points clearly forward, not hurriedly and not with such exceeding brevity as to lose myself in obscurity.

Of the various agents that may fairly come under consideration, I will confine what has to be said, in the first instance, to substances which produce general insensibility, and of these I will name none that have not been tried, and for some valid reason or other been found wanting in some essential quality.

Nitrous oxide and sulphuric ether, in their respective ways, represent what are considered to be the physical requirements of agents for the production of anæsthetic sleep. The idea now is well nigh universal, that the agent must either be a gas, as nitrous oxide is, or a liquid very easily transformable into vapour, as ether is. These, I say, are conceded points, and are based on the belief that the best and readiest mode of producing the general sleep of insensibility, is letting the narcotic enter the system by the channel of the lung; by presenting to the blood as it flows over the lung from the right to the left side of the heart, some substance which the blood will absorb and will carry direct to the nervous centres in quick and steady stream. It is true we can produce the effect we want in another way, by another mode of entrance. For example, I have recently found it quite possible and easy to put an animal profoundly under chloroform, by inserting that substance, in its fluid state, beneath the skin with a fine needle, and this method could be applied to any soluble narcotic; but as a general principle, it is not so good a method as that of administering by what is called "inhalation."

We may put, therefore, in classification all the modern agent

we employ under two general heads, of GASES and VOLATILE FLUIDS, and I will set the most reliable substances in this order, adding to the name of each substance its chemical composition and physical characteristics.

TABLE I.—GASES.

Name.	Chem. Comp.	Density H=1.
Nitrous oxide	N_2O	22
Methylic ether	C_2H_6O	23
Chloride of Methyl	CH_3Cl	25·25

TABLE II.—VOLATILE FLUIDS.

Name.	Chem. Comp.	Boiling Point.	Vapour Density H=1.
Ethylic ether	$C_4H_{10}O$	92° F.	37
Bichloride of ethylene (Dutch liquid)	$C_2H_4Cl_2$	175° F.	49·5
Bichloride of methylene	CH_2Cl_2	104° F.	42·5
Terchloride of formyl (chloroform)	$CHCl_3$	142° F.	59·75
Tetrachloride of carbon	CCl_4	172° F.	77

I might expand these tables very largely. To the gases I might add carburetted hydrogen, light and heavy; carbonic acid, carbonic oxide, and hydrogen gas. To the liquids I might add methylic alcohol, chloride of ethyl, hydride of amyl, amylene, and many others, not omitting turpentine, which, at a push, has actually been administered successfully to the human subject. It is better to take simple types of those agents which are most direct in action, or most in use at this hour. Respecting those agents noted in our tables, it may be well to say of them at once, that, although they are the best up to the present time discovered, not one of them is to be considered perfect. All the gases are faulty because, being gases, they are practically unmanageable for ordinary application. Nitrous oxide gas, moreover, although it produces insensibility, causes, at the same time, darkening of the arterial blood, painfully rapid breathing, a countenance terrible to behold, and imminent approach to death. The liquids, on their side, are practical for administration, but more or less faulty in action.

At a future day, keeping to the same text, I will endeavour to point out the relative advantages and disadvantages of each of these agents in detail; and will try to explain what is wanted in an anæsthetic agent to make it in all respects perfect; and when I say perfect, I mean so perfect, that the removal of sensibility, and thereby the prevention of suffering by it, shall be attended with no anxiety, and with no sign or symptom that shall excite in the operator and administrator the anxiety which every administration, hitherto, has called forth.

THE DEPOSITS OF THE ATLANTIC IN DEEP WATER, AND THEIR RELATION TO THE WHITE CHALK OF THE CRETACEOUS PERIOD.

BY PROFESSOR D. T. ANSTED, M.A., F.R.S., For. Sec. G.S., &c.

[PLATE LV.]

The origin of chalk, a substance very different from other limestones with which it agrees nearly enough in chemical composition, has always been a subject of enquiry with geologists, and the theories suggested have been numerous and varied. It was not till a necessity arose for determining the depth of the sea bottom across the Atlantic, preparatory to laying the cable for the Atlantic telegraph, that deep soundings were systematically taken between Europe and America; but when this was done a close relation was detected, both in appearance and organic contents, between chalk and the mud dredged up from the Atlantic bottom in deep water. Something like evidence has thus been obtained as to the possible conditions of deposit of the upper member of the cretaceous series.

White chalk is singularly uniform as a rock in all those parts of the world where it has been discovered. It is not always of the same degree of hardness, and it occasionally differs in colour as well as texture from the white chalk of the south of England that may be regarded as the type, but it is everywhere a homogeneous fine-grained rock showing few or no marks of stratification except on a large scale, and containing in comparison with many other rocks few organic remains of sufficient magnitude to be seen by the unassisted eye. It ranges from the north of Ireland in the north-west to the Crimea in the south-east, a distance of nearly 1,400 miles; and from the south of Sweden in the north almost as far as the Pyrenees in the south, a distance of nearly 1,000 miles. In England it approaches 1,000 feet in thickness, and in southern Russia it is as much as 600 feet. Throughout this wide space it retains its essential character as a rock, and cannot readily be mistaken

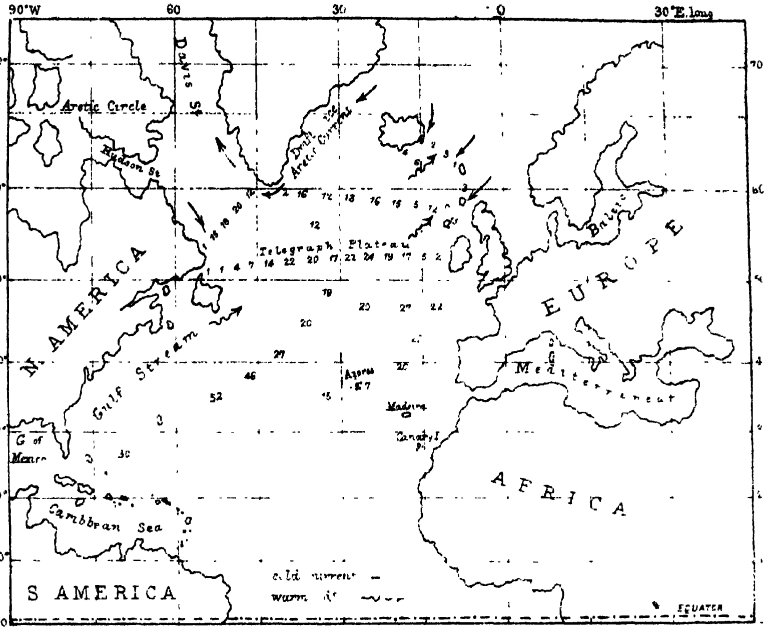
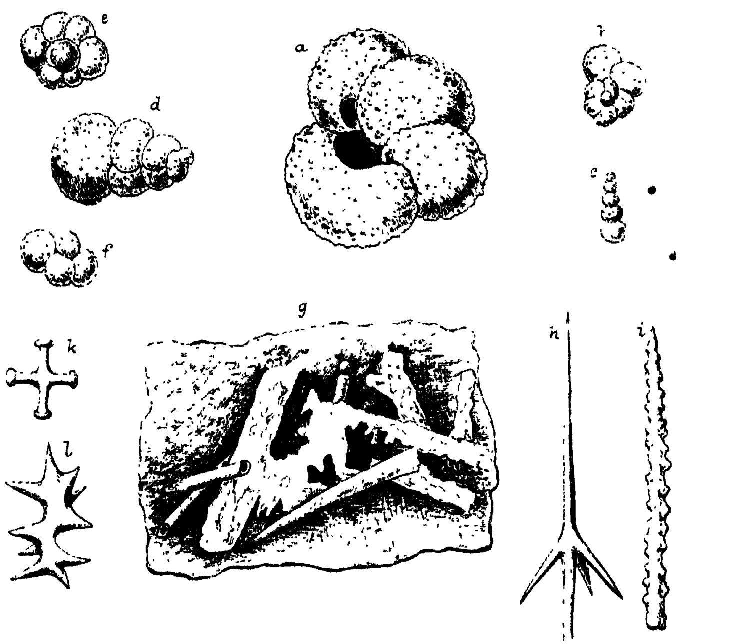


Chart of the North Atlantic Canal.



Tuffen West sc

W. West imp

Atlantic Sea-bed and its contents

for any other limestone. Although hard in Central Europe it is almost as soft as in England when met with in Russia.

In the absence of positive knowledge as to the nature of deep sea mud Sir Charles Lyell and others, struck by the interesting observations of Mr. Darwin on coral reefs, and by some resemblances between chalk and the fine mud at the bottom of coral lagoons, had set men's minds thinking how far the coral animal and the fishes that feed on coral might be responsible for this variety of calcareous rock. Although the idea was plausible, further observation has shown that it is rather the Oolites than the chalk deposits that have been thus formed. Lagoons enclosed by coral receive beyond a doubt a vast quantity of fine mud that might represent chalk, but this is usually mixed with a far greater variety of fossils, especially of corals and bryozoa, and a more plentiful sprinkling of echinodermata, crustacea, and even shells than are at all usual in chalk.

It was not till the year 1858, when the British steamer Cyclops, commanded by Captain Dayman, following nearly in the course of exploration pursued by Lieutenant Berryman in the United States steamer Arctic, was systematically employed to take deep soundings and determine the nature of the sea bottom, that the real material at the bottom of deep water in the Atlantic was obtained and brought to the surface in quantity sufficient to permit of accurate observation. It was no easy or simple matter at first to lift even a small quantity of the scrapings of the sea bottom through 10,000 feet of water, but this has now become an ordinary and necessary result of every deep sounding.

It is no part of the object of the present article to repeat the accounts already frequently given of the methods by which deep soundings and dredgings in deep water have been accomplished. Various methods have been adopted, with various but gradually increasing success, until at last actual dredging has been carried on and nearly two hundredweight of the sea bottom, with its living inhabitants and the skeletons of the recently dead, has revealed a submarine life at a depth of more than 2,000 fathoms, not less varied nor less numerous, and certainly not less interesting, than that which may be studied within the few feet of tidal range near the shore. These results have been obtained in many parts of the North Atlantic, and year by year this ocean bed has been more and more the subject of investigation.

It may be well to remind the reader that between the land of Western Europe (including the British islands) and the east coast of North America (including Greenland), there is a space of nearly 1,500 miles of ocean, for the most part more than 1,000 fathoms deep, but imperfectly interrupted by banks. These banks rise into land in Iceland and the Faroe islands,

and probably approach near the water surface somewhere about latitude 55° N. and longitude 30° E., where according to old accounts there was once land. The telegraph plateau, as it is called—the line of sea bottom on which the first Atlantic telegraph was successfully laid—lies a little to the south of this bank and is much deeper. To the north there is the Arctic Ocean covered most part of the year by ice, and only communicating occasionally with the Pacific Ocean by narrow choked up channels. To the south there is the Gulf Stream, a current of warm water, at the surface certainly approaching mid-Atlantic, whatever may be its extension towards the shores of Europe. Warm water is brought in this way across the Atlantic steadily, incessantly, and in large quantities, and it is quite impossible that the drag of this great stream-current should not even by mere friction produce a current of some depth. It is certain that in some places the warm current reaches the actual sea bottom.

Recent observations have shown that besides this warm current reaching the latitude of 50°, the part of the Atlantic canal we have alluded to above is affected by certain causes which produce real and well-defined currents, some near the surface, some near the bottom, some warmer than the mean temperature of the air above, and some cooler. There are broad and deep warm currents running from south-west to north-east, and other broad and deep cold currents running from north-east to south-west, and this complication of currents, influenced, there cannot be a doubt, by the form of the sea bottom, appears to have produced certain natural history results which are of the most extraordinary significance with reference to the geological question we are considering. It is in this respect, perhaps more than any other, that the value of recent observations concerning deep sea temperature must be measured.

That there is an important arctic drift running down along the coast of Greenland, conveying ice occasionally, and for some cause turned northward at the southern extremity of Greenland into Davis's Straits, has been long known. It is this current that renders Greenland almost uninhabitable, in latitudes where in Norway and Sweden we have a very pleasant climate and a large population. The cold or arctic current runs between the west coast of Iceland and the east coast of Greenland. (See Chart in Plate.) There are also powerful currents at a very low temperature proceeding through Hudson's Straits, entering the Atlantic near Newfoundland, and crossing the warm Gulf Stream.

One part of the great arctic current running along the east coast of Iceland passes down into the Atlantic to the south-west and is there soon lost. Another part, however, still further to

the east, runs between and past the Faroe islands and the bank called Rockall; while a third cold current passes along the coast of Scotland. All these are in comparatively shallow water, rarely exceeding 1,000 fathoms, and all are strictly limited by natural barriers.

It is not to be supposed that there can be this continual drain of cold water from the arctic seas without some supply being drifted in by counter currents. No doubt there is a very large and constant evaporation from the mid-Atlantic, but there is also a heavy rainfall and a large number of streams pouring into the ocean the drainage both of Europe and America. There are in fact two deep warm currents, one between Iceland and the Rockall and Faroe banks, and the other between these banks and Scotland both also shut in by natural barriers.* These warm currents proceed from a deep central warm tract of ocean lying to the east and north-east of the limit of the Gulf Stream, and it is this warm tract, with its branches northwards, that affords in its floor the conditions apparently most favourable for certain deposits that have lately been described as resembling chalk.

The deep sea explorations of late years carried on with improved means of determining bottom temperature, have made it clear that there are well marked deep areas that possess a warm temperature, not very far from other areas where the bottom temperature is uniformly low. It is a further result of the same work that over the cold areas generally the bottom is sandy, often of volcanic sand, and these parts exhibit a considerable variety and sometimes a great wealth of animal life; but the organic forms but a small proportion in comparison to the inorganic element. Here, however, a vast multitude of the curious worm-like marine animals assuming a coat of agglutinated grains of sand have been met, and the multitude of new species obtained in recent soundings is so great that there is a difficulty in finding names for them. Besides annelids, sponges, echinoderms, mollusks, and crustaceans have been found, so that these regions are, on the whole, exceedingly rich. In one spot alone where the dredge brought up but little, a tangle of hemp lifted at one haul many thousand specimens of a single form of echinus.

On the other hand, in the warmer areas the deposit of mud consisted exclusively of a peculiar material called *ooze* or *oaze*

* It was found in the recent (1869) expedition that a difference in bottom temperature, between 32° and 47°, existed at points only eight or ten miles distant beneath a uniform surface temperature of about 52°. In such cases the cold area was paved with barren sandstone having a scanty boreal fauna, while in the adjacent warm area the bottom was mud, with an abundant temperate fauna.

by nautical people—a soft mealy substance, having the appearance of mud but of very close texture. This substance is described as remarkably sticky, having been found to adhere to the sounding rod and line through its passage from the bottom to the surface, in some instances more than 2,000 fathoms. When a little of this mud is taken out and thoroughly dried it becomes white or reddish-white and (though less white) it closely resembles very fine chalk.* Fully nine-tenths by weight of this deposit was estimated by Professor Huxley to consist of minute skeletons of Foraminifera, composed of carbonate of lime. Examined under the microscope it presents a vast multitude of exceedingly minute granules and fragments, and a certain proportion of some clear mineral, perhaps quartz, perhaps volcanic sand. The granules and fragments are almost without exception referable to one species of Foraminifera known as *Globigerina*, traced through a complete series of gradations from less than one-thousandth of an inch in diameter (when it consists of one or two cells) to more than one-sixtieth of an inch. The general appearance of the complex forms consisting of several cells is given in the plate annexed. Among the mud that does not consist of fragments of Globigerinæ are fragments of diatoms and indications of sponges. When these remains were first found to form a large part of the mud in depths of upwards of 2,000 fathoms, and it became clear that they were uniformly spread over a large space, the knowledge of the existence of animal life in deep water was so small that many naturalists speculated as to the possibility that the accumulation consisted only of the skeletons—the animals themselves having lived near the surface. Subsequent observations have shown that there is no known limit of depth at which animal life ceases. There can hardly be a doubt that the Globigerinæ are the natural inhabitants of the ocean floor at all depths under favourable conditions, and that these conditions consist of a warm, or comparatively warm, sea bottom. It is also certain that a warm sea bottom is a local condition nearly independent of latitude—absent in some seas whose surface water is very warm, and present in others where the surface and moderate depths are very cold.

In the dredging expedition of 1868, a large quantity of Globigerina mud was lifted always from the deep warm bottoms; but this mud was found to include animal life of higher types and was everywhere permeated by a peculiar glairy organic substance, regarded by Prof. Huxley as an intermediate condition between plant and animal life, and called by him *Bathybius*. The nature and true position of this remarkable substance is not

* Huxley's *Appendix to Capt. Dayman's account of Deep-Sea Soundings in H.M.S. Cyclops.*

yet satisfactorily made out, but the bottom temperature that appears most favourable to the rapid multiplication of these organisms would seem to be about 45°, and the depth of water at least 2,000 fathoms.

It is worthy of notice in reference to this, as in other matters connected with deep sea dredging, that the first suggestions and the germ of much of the theory that has been lately advocated by the distinguished naturalists who were selected by the Council of the Royal Society to conduct the enquiry for which sounding and dredging ships have been granted by Government, was due to Dr. Wallich, who accompanied Captain McClintock in the Bulldog, and the results of whose observations were published in various ways between 1860 and 1862. In the year 1860, after his return from the Bulldog expedition, Dr. Wallich pointed out the absolute confirmation of previous observations as to the presence of animal life at great depths, which were till then doubtful owing to the absence of certainty as to actual depth, and a belief that the remains found might have sunk from the surface. That the minute foraminifera, and even other larger and more complex forms of life, dwelt in deep bottoms was proved by the bringing up of a living star-fish from 2,000 fathoms, and perhaps yet more conclusively by examining the contents of the stomachs of the animals coming from the bottom and only recently dead. Dr. Wallich, in his account of the natural history results of the Bulldog expedition, called attention to all these facts, suggesting their importance as pointing to the possible origin of chalk, distinctly advocating the view that there may exist in these deeper parts of the sea an intermediate form of protoplasmic life neither animal nor plant, but common to both, and urging strongly the necessity of further exploration. Dr. Wallich afterwards endeavoured through the Council of the Geographical Society, to induce the Government to place a ship at the disposal of qualified naturalists for the prosecution of these researches, and employed the same arguments as those which were afterwards, and more successfully, used by the Council of the Royal Society with the same object. It is a matter of regret that the valuable services of Dr. Wallich in this matter have been grudgingly admitted in the published reports of the Royal Society Committee, and that Dr. Wallich himself has not received from the Society that recognition which his anticipation of their movement in the matter certainly deserved.

There are two or three things to be explained in reference to the origin of chalk, and among them the occasional bands of flint are perhaps among the most important. As white chalk is nearly pure carbonate of lime, so flint is generally almost pure silica. The flint is present in occasional lumps of strange

grotesque shape, or in thin films follows planes of stratification. It also occupies vertical cracks and joints. Besides these, certain large cylindrical hollow fossils, growing one over another to the height of some yards and apparently rising contemporaneously with the chalk deposit, are entirely formed of flint. All these flints, almost without exception, show, when microscopically examined, marks of organic structure referable generally to sponges and spongiform bodies. Some idea of their appearance under the microscope may be obtained by referring to the illustration in the annexed plate (fig. g). The explanation of chalk, whatever it may be, must include a reasonable account of these contents, but as they are in some places very abundant and in others almost absent, it is clear that their presence is not absolutely necessary. That they are due to organic agency there cannot be a doubt, but whether they represent the protoplasm or *Bathybius* of Prof. Huxley, always at hand, and which may be supposed to silicify rapidly under certain conditions, or whether they are simple results of a quantity of silica introduced from time to time from without by thermal springs accumulating round spongy bodies containing siliceous spicules (see Plate figs. h, i, k, l) there is at present no sufficient evidence to determine. Parts of the upper chalk, as we have seen, also contain fragments of bryozoa, radiated animals, crustaceans, mollusks of all kinds, fishes, and reptiles: they even contain fragments of wood and a few pebbles. All these are consistent with the composition of the rock as a submarine mud deposited in open and deep water, for the recent investigations may be said to have cleared up all doubt as to the possibility of life of all known kinds in water of more than 2,000 fathoms, and the few foreign substances are no doubt the result of accidental drift. The only requirements of life, apparently indispensable both in water and air, are the free access of light and of certain gases and sufficient heat, and a temperature twelve or fifteen degrees above the freezing point of water, appears to be sufficient for the rapid growth and multiplication of the Foraminifera, while a lower temperature is actually richer in species though infinitely more barren in the number of individuals.

It is no doubt necessary before we admit that chalk is foraminiferous mud that we should know its composition, and this has not yet been determined on a large scale. The result of a very simple analysis would seem to show that in some cases at least the foraminiferous mud or oaze contains more than 90 per cent. of carbonate of lime. Other examples, recently submitted by Mr. Gwynn Jeffreys, make it appear that not much more than 50 per cent. of some samples is of this material. It is clear that a few isolated analyses are not sufficient in a question of this magnitude, and thus a certain amount of doubt

rests as to the material. This will no doubt be removed before long by the assistance of the chemists.

Assuming, as is certainly probable, that foraminiferous mud is generally a nearly pure calcareous substance mixed with a small proportion of silica and some trace of organic matter, there can hardly be a doubt that its colour, texture, general nature, and contents, do correspond singularly with those of white chalk, and render it not unlikely that all those parts of Europe and Asia where chalk is now found were, during the cretaceous period, at least 8,000 feet below the sea. As some of them form hills of considerable height, a rise of at least 10,000 feet in places is required for the appearance of the rocks in their present position. There is nothing in this assumption in any way opposed to what is already known regarding the elevation of the earth's surface, during the tertiary period in Europe, Asia, and Africa. On the contrary, a change of level to this extent is indicated by the great mountain chains, all of which are modern, and all of which have probably been higher rather than lower than they now are during the tertiary period.

The necessity of a warm sea bottom during the accumulation of foraminiferous mud, and the fact that warm sea bottoms are quite independent of latitude and have nothing whatever to do with the climate of adjacent land, are perhaps among the most important of the recent investigations. The influence of depth on the nature and homogeneity of deposits is also remarkable. That animal life exists in all its activity and without any check in the deepest recesses of the ocean has now become more than probable, and that geologists must look to bathymetrical conditions and conditions of bottom temperature far more than they have hitherto done in considering the circumstances under which deposits have been made is perfectly clear.

As limestone of all kinds is apparently due to a large and rapid growth of animal organisms, while arenaceous deposits are chiefly abundant where life, if present, exists under less favourable conditions, and as heat appears to play an important part in rendering the sea bottom favourable or otherwise for rapid accumulations of calcareous matter, it becomes evident that the direction of marine currents is even more than has yet been recognised a prime agent in all geological causation. The diversion of the warm currents from their present course towards the north-east between Iceland and the Hebrides would at once check or cause to cease the accumulation of foraminiferous mud throughout the temperate latitudes of the north Atlantic. The depression of central Europe to become the bottom of a deep sea with an outlet into the Pacific would convey the warm currents eastwards, and produce accumulations which might rival the chalk in magnitude. On the other hand,

the existence of a barrier of land west of Ireland, extending southwards from Iceland, would cut off the warm current from the Gulf of Mexico and produce a boreal fauna in the sea, just as a considerable extension of land within the arctic circle would chill the climate of all northern European land, and bring down an icy cap to the Alps and Pyrenees.

We have only to look at the soundings already made, and compare the facts determined as to the depth of the Atlantic, to see how easily a comparatively small change in the relative level of the sea bottom must, by influencing the bottom temperature, modify and entirely alter the nature of the deposits. For this purpose a glance at the annexed chart (see Plate) will be sufficient. It will there be seen that undulations along the central line of the Atlantic canal, must divert currents which now convey warm water into latitudes very near the Arctic circle, while corresponding undulations across the ocean would affect such currents very little. On the other hand, it is well known that the great land undulations in our hemisphere have been rather in a direction parallel to the equator than from north to south, so that as there is a volcanic axis from Iceland southwards towards the African coast, indicated sometimes by actual eruption and very frequently by earthquakes, the movements are on the whole at right-angles, tending to separate still further the eastern from the western hemisphere, as the northern part of the old world is already separated from the southern.

Whatever may be the value of speculations of this kind, there can be no doubt of the necessity of further active explorations of the sea bottom, accompanied by observations of bottom and intermediate temperature in all seas. Hitherto the north Atlantic canal has alone been examined with any approach to system, and even there but a very small area has been looked at. The parts of this ocean to the south of the telegraph platform should be followed and soundings taken, with a view to discover the direction of the banks between which the warm stream runs towards the Arctic Ocean. Systematic search to connect, if possible, Iceland and the volcanic islands of the Atlantic off the coast of Africa, and thus to discover whether the fauna east and west of this line are identical, is greatly needed. A similar search northwards, following the warm channel in that direction, could not fail to yield important information, while the pursuit of the cold current bringing icebergs to its southern extremity after crossing the Gulf Stream might yield results little anticipated. The whole subject is one full of interest and promising a long continuance of important discovery. There is ample field for all, and it is hardly possible to imagine a more noble use of the resources in the possession of some of our wealthy owners of steam yachts, than a systematic

pursuit of dredging operations in the little visited parts of the ocean. Who can tell what treasures may reward those who first dredge in the Sargasso sea, or in the tropical waters of the Caribbean Sea and the Gulf of Mexico and in the Atlantic between Africa and America? That each cast of the dredge would bring up treasures of some kind is certain, provided only that there be a competent naturalist at hand to direct the work and record the results. Governments may and will vie with each other in this work. Whether the singular disinclination of our own Government and of naval commanders to employ ships for natural history work may be partly overcome, as might be supposed, by the recent success of the application of the Royal Society Council remains to be seen, but the American and Danish Governments have already intimated their intention of doing their share. The French are not likely to be far behind. There is ample room for all, and no fear need exist of any clashing of interests. More important facts than those resulting from widely-extended sounding and dredging operations can hardly be obtained, and at present the work done is as nothing compared with that which remains. There should be no jealousy in such a matter either among individuals or nations.

EXPLANATION OF PLATE LV.

Chart of the north Atlantic canal, showing several lines of soundings, marked in hundreds of fathoms, and the set of various stream and drift currents.

a. Recent Globigerina from the Atlantic sea bottom (*Wallich*).

b, *c*. Varieties of form of Globigerina from shallow water.

d, *e*, *f*. Globigerinæ from the chalk.

g. Structure of flint highly magnified, showing sponge spicules.

h, *i*, *k*, *l*. Varieties of form of recent sponge spicules.

WHAT IS WINE?

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IN a former communication we have seen how, in the course of fermentation and subsequent maturing, the comparatively speaking simply constituted *must* is changed into the highly complex mixture wine. We will now consider somewhat more fully the quantitative relation of some of its most important constituents. Before doing so, however, it may be well once more to recall to mind the various substances hitherto detected in wine, and to contrast them, in a tabular form, with those found in the *must*.

Must contains		Wine contains	
water		water	
grape sugar	10 to 30°/o	grape sugar	0·0 to 6°/o
fruit ,,		fruit ,,	
malic acid		ethylic alcohol	5 to 22°/o
tartaric ,,		propylic ,,	
racemic ,,		butylic ,,	
albumenoid substance		amylic ,,	
vegetable mucus		etc. etc.	
essential oils		malic acid	0·3 to 0·8
extractives		tartaric ,,	
mineral substances		racemic ,,	
tannin	from the skins and kernels	acetic ,,	
colouring matter		succinic ,,	
fatty oil		formic ,,	
		propionic,,	
		butyric ,,	
		etc. etc.	
		ethers of foregoing alcohols and acids	
		glycerine	
		aldehyd	
		carbonic acid	
		ammonia	
		trimethylamin	

Must contains	Wine contains
	ferment oils
	albumenoid substances
	vegetable mucus
	colouring matter
	tannin
	fatty oil
	essential oils
	extractives
	mineral substances 0·15 to 0·6

Most of the above given substances are contained in every wine, although in very variable proportion, while some of them are found only in certain kinds; such, for example, as colouring matter, tannin, racemic acid, formic acid, aldehyd, &c. &c. The character of a wine will, of course, depend upon the relative proportions of all the various constituents present, and a glance at the long list of substances found will suffice to show, that their intermixture in various proportion may readily give rise to an almost infinite variety. Some of these substances admit of accurate quantitative estimation, and their relation to certain characters of the wine it is thus, to a great extent, within the power of chemical analysis to determine. Many of them, however, do not as yet admit of being thus estimated; and, even if we are content with simply showing their presence, it is frequently necessary to operate on large quantities of the wine. Those constituents of a wine (as the alcohol, sugar, acids, mineral constituents, &c.) on which its physiological action chiefly depends admit of accurate estimation. Chemical analysis will, therefore, give us valuable information regarding its dietetic and medicinal qualities, and will show us its strength, as well as whether it is pure and sound, or the reverse, and whether genuine or factitious. On the other hand, most of the constituents upon which the quality of the wine—i.e. taste, and therefore market value—depends, are among those not admitting of accurate estimation. Chemical analysis alone, therefore, will afford, generally speaking, little or no information to guide us in this respect.

In entering upon a closer examination of their various constituents, it will be well to bear in mind the distinction, previously given, between the natural wine on the one hand and fortified or brandied wine on the other.

Pure, natural wine, is the fermented juice of the grape, without any addition or subtraction. Such a wine, when made of good ripe grapes and in favourable seasons, will exhibit all the vinous qualities in the highest perfection, and with the most complete concentration of the various constituents of the *must*. Not having been diluted by addition, or suffered loss by

removal, such changes only occur in it as are brought about by fermentation itself; such as, for example, the conversion of the sugar into alcohol, &c., the solution of colouring matter, precipitation of tartar, &c. &c. The wine, moreover, having once thoroughly fermented, is not very liable to further change in that direction, and is therefore wholesome and of excellent keeping quality. In fortified wine, on the other hand, fermentation has not been allowed to run its regular course, but has been checked prematurely by the addition of spirit, by which the strength of the wine is brought above the limit within which, as we have learned, vinous fermentation is possible. The wine, therefore, although of good keeping quality as long as its strength is unimpaired, is liable to ferment whenever the preserving power of the spirit added may be destroyed by dilution, as, for example, when the wine is drunk.* It is on this account less wholesome than the pure wine. The concentration of the wine also is lessened by the addition of the spirit, in consequence of which it contains relatively less constituents of the grape than the natural wine. The fortified wine certainly often contains no inconsiderable amount of sugar which has been preserved from fermentation, and this induces the belief that greater concentration exists; but this sugar has only been preserved at the cost of a great addition of spirit, whereby all the other constituents have been diluted.† This sugar, moreover, if required, could be obtained much more cheaply as cane sugar. A bottle of port wine with 4% of sugar (and this is a sweet port) contains about 1 ℥ of sugar, of the value of one-third of a penny.

* Medical men will probably find in this an explanation of the fact that even good fortified wines are liable to produce flatulency and dyspepsia in persons with weak digestion. The wine is drunk, either already diluted with water, or is subsequently diluted by the gastric juice; the protecting power of the spirit is destroyed, and fermentation and consequent acidity are the necessary result.

† An example will, perhaps, make the case more clear. Supposing a *must* from which port is to be made to contain 28% of sugar—and this would be a very good *must*—it would be capable of producing a wine having a strength of about 13%. If now 4% of sugar are to be retained in the fortified wine, the strength produced by fermentation will barely reach 10%, and, as the wine has to be brought up to say 19% (42 degrees proof spirit), about 30 bottles of proof spirit have to be added to every 100 bottles of wine. All the vinous qualities before contained in 100 bottles are now distributed through 130 bottles, or one bottle of the fortified wine contains only about three-fourths of a bottle of real wine. The dilution will be somewhat less if stronger spirit is employed, but the general fact will remain the same. If, as is frequently the case, grain spirit or even potato spirit is used for admixture, dilution is not the only drawback, inasmuch as substances quite foreign to the genuine wine are, in that case, introduced in addition.

Let us now turn to the consideration of the chief constituents of the wine, more particularly of such as admit of quantitative estimation. The most important of these is

Alcohol.—Ethylic alcohol, or spirit of wine. As before explained, the sugar of the *must* breaks up, in the course of fermentation, chiefly into carbonic acid and alcohol, the former escaping into the air, the latter remaining in the wine.

$$\underset{\text{sugar}}{C_6H_{12}O_6} = \underset{\text{alcohol}}{2(C_2H_6O)} + \underset{\text{carbonic acid}}{2CO_2}$$

By submitting the wine to distillation, this alcohol can be separated from the rest of the constituents and estimated.

Generally speaking, all wines which are simply the fermented juice of the grape (natural wines) contain from 60 to 130 parts by weight of alcohol in 1,000 volumes of wine (6 to 13%). With less than 60 parts the wine is scarcely drinkable, and more than 130 parts cannot, except in a very few rare cases, be contained in a natural wine. Firstly, because grape juice seldom contains the requisite amount of sugar for the production of more alcohol than this; and, secondly, because such an amount represses, or altogether stops, fermentation, and so protects the rest of the sugar from decomposition. A wine of more than 13% of alcohol is almost certainly a fortified wine; the great majority of natural wines contain less than 12%.

In fortified or brandied wines the strength, of course, depends on the amount of spirit added, and on the original strength of the wine; it may, however, be taken as ranging between 120 and 220 parts per 1,000 volumes (12 to 22%), being rarely above and rarely below that amount.

Besides this ethylic alcohol wine contains, as we have seen, small proportions of a number of other alcohols; as, for example, propylic, butylic, amylic alcohol, &c. &c. These alcohols are closely allied, chemically, to ethylic alcohol; indeed, they are termed alcohol on that account, and are, like it, produced during the fermentation of sugar. They have, undoubtedly, a great influence upon the bouquet of the wine, and perhaps also upon its physiological action. At present they do not admit of accurate quantitative estimation.

Acids of Wine.—All wine contains a greater or less proportion of free acid, and possesses, therefore, an acid reaction and a sour taste; but whilst the degree of acidity, as measured chemically, depends only upon the proportion of acid present, the acidity, as judged by the palate—the more or less sour taste of the wine—is greatly influenced by the nature of the acid and the presence or absence of other constituents. Thus a wine of low alcoholic strength, and without sugar, will taste unpleasantly sour, with a degree of acidity which, in a strong and sweet

wine, would scarcely be perceived, the sugar and spirit in the latter case masking the sour taste. This effect should never be lost sight of in judging of the relative acidity of natural and fortified wines. Good wines of either sort contain in reality pretty nearly the same amount of acid, but in the fortified wine the sour taste is, as above described, less prominent. It is, however, a great mistake to suppose that, because the acid is masked to the taste, that it is not present; and any injurious effects that may be supposed to follow its use in the one case will most assuredly also follow in the other.

The acids of wine may be conveniently divided into two classes: firstly, non-volatile or fixed acids, such as cannot be distilled over or expelled by evaporation without decomposition; and, secondly, volatile acids, which can be distilled or expelled.

The acids of the first class are all derived directly from the grape juice, but are present in the wine in a different proportion to that in which they were contained in the *must*; for the salts of some of these acids (those of tartaric acid, for example) are less soluble in the wine than in the *must*, and are consequently precipitated during fermentation. The chief acids of this class are malic acid and tartaric acid, the former usually predominating. In pure natural wines, made of ripe grapes, tartaric acid is probably never absent; but its amount varies considerably, though rarely or never equalling the amount of malic acid present. In some wines part of the tartaric acid is replaced by the closely allied racemic acid. Generally all the tartaric acid contained in a wine is present in the form of an acid potassium salt (cream of tartar); only one half of it contributes, therefore, to the acidity of the wine, one half being neutralised by the potassa.

In all fortified wines, plastered wines, or wines otherwise subjected to artificial treatment lessening the acidity, there is considerably less tartaric acid; indeed, in such wines it is frequently totally absent.

The volatile acids have all been formed during or after fermentation; they are produced by the action of oxygen on some of the constituents of the wine, representing, so to speak, the first beginnings of its ultimate destruction. In small quantity they are indispensable to the wine, being chiefly instrumental in the production of its flavour and bouquet. When present, however, in large quantity, they are very objectionable, affording undoubted evidence of maltreatment of the wine, and rendering it very liable to turn sour altogether, i.e. to become vinegar. Such wines, therefore, should be carefully avoided. Among the volatile acids present the acetic is found in greatest proportion, and is formed by the oxidation of the ethylic alcohol. Besides this acetic acid there are always small quantities

of other homologous acids, such as propionic acid, butyric acid, &c. &c. It is to these latter that the production of the bouquet is chiefly due.

In good sound wines the total amount of free acid, that is, acid not combined with or neutralised by an alkali, varies from three to six parts per thousand, calculated as if all the acid were tartaric acid. This, as before stated, is not strictly correct, but it is the conventional method of expressing the acidity of a wine. In the case of white wines, not fortified, not more than about one-fourth of the total acidity should be due to volatile acids; in the case of red wines, or fortified wines, the proportion of volatile acid is generally higher, but should not, even in these, amount to more than about one-third of the total free acid.

Sugar in Wine.—Grape juice contains two kinds of sugar in equal proportion—grape sugar and fruit sugar. Both these sugars are capable of fermentation, both are destroyed by being heated with a solution of an alkali, and both reduce copper salts from their warm alkaline solutions. The first, however, is crystallisable, and turns the plane of polarised light to the right; the second forms an uncrystallisable syrup, and turns the plane of the polarised ray to the left about twice as much as the other turns it to the right. A mixture of both in equal proportion, such as is found in the grape, will therefore turn the ray about half as much to the left as pure grape sugar turns it to the right. This same mixture of sugars is produced by the action of yeast, or acids, on cane sugar, and is then called invert sugar, since, as above explained, it turns the plane of polarised light to the left, cane sugar turning it to the right. Cane sugar then, when added to *must*, or wine, very soon becomes changed into invert sugar—a mixture of sugars identical with that found in the grape—and a few weeks after its addition is no longer found as cane sugar. During the fermentation of the *must* grape sugar chiefly is destroyed, so that the sugar remaining in the wine is in greater part fruit sugar.

By the action of sulphuric acid on starch, a sugar is produced chemically identical with grape sugar (not with the mixture of sugars as found in the grape), and this is frequently employed in the adulteration of *must* and wine.

Natural wines, when more than a few years old, contain, as a rule, little or no sugar. Young wines of this class not unfrequently, however, retain an appreciable quantity of sugar, amounting in some rare instances even to 6%, though more usually to 0·5 or 2% only. As long as the wine contains this amount of sugar, it is very apt to enter again into fermentation, and must therefore be kept for some years in cask, and in a cool cellar, before it can be safely bottled. In time this sugar gradually diminishes, even without any decided fermentation,

and in the course of several years sinks down to a few tenths of a per cent. or so, and the wine is then fit for bottling. It is only in rare cases that a natural wine can be safely bottled when it contains more than 1% of sugar.

Fortified wines, the fermentation of which has been stopped by the addition of spirits, generally contain more sugar, ranging from 0 to 5%, and in a few liqueur wines rising even to upwards of 25%. The amount of sugar found in these wines depends, firstly, upon the amount left when fermentation was stopped; and, secondly, upon the quantity added, as so-called saccharine, to the finished wine, this latter being very frequently done in this country. Sometimes also, a portion of evaporated *must*, which has not been subjected to any fermentation, is added to the fortified wine. It is on this property of the spirit of preventing the fermentation of sugar that its chief use to the wine grower and merchant depends, as it makes even young wine at once a marketable article. The buyer is then obliged to keep the wine for many years in his cellar, to allow the injurious effects of the added spirit to disappear, and let the wine once more become a pleasant drink.

In all pure or simply fortified wines by far the greater proportion of the sugar present is, as before explained, fruit sugar, the rest being grape sugar; in such, to which inspissated *must* or a solution of cane sugar (saccharine) has been added, the proportion of grape sugar is greater, though still bearing the fruit sugar in excess. In wines in the preparation of which sugar of starch has been employed grape sugar is the predominating kind.

Compound Ethers.—We have before seen that whenever an alcohol (or several alcohols) and an acid (or several acids) are mixed, the formation of what are termed compound ethers commences. A compound ether may be looked upon as a salt of an acid, with an alcohol radical analogous to the ordinary mineral salts of the acid.

$$\text{as } \underset{\text{potash}}{(K)HO} + \underset{\text{acetic acid}}{C_2H_4O_2} = \underset{\text{acetate of potassium}}{C_2(K)H_3O} + \underset{\text{water}}{H_2O}$$

$$\text{and } \underset{\text{alcohol}}{(C_2H_5)HO} + \underset{\text{acetic acid}}{C_2H_4O_2} = \underset{\text{acetic ether}}{C_2(C_2H_5)H_3O} + \underset{\text{water}}{H_2O}$$

Or, expressed in words, hydrate of potassium (potash) and acetic acid yield acetate of potassium and water; and hydrate of ethyl (alcohol) and acetic acid yield acetate of ethyl (acetic ether) and water.

The production of compound ethers commences as soon as the acid and alcohol come together; it takes place rapidly at first, very slowly towards the end. If water be absent, and the water produced during the reaction is removed, the process goes

on until either the whole of the acid or the whole of the alcohol has been converted into compound ether. The presence of water, however, prevents this complete etherification; the more water present, the smaller the proportion of ether formed from a given quantity of alcohol and acid. Under these circumstances, then, the amount of alcohol converted finally into compound ether depends upon the proportion of alcohol, acid, and water present, but is independent of the nature of the alcohol and acid.

Whatever be the proportion in which these three substances are mixed, after the lapse of a longer or shorter time there will have been produced a certain amount of compound ethers, the rest of the alcohols and acids remaining free; beyond this no more ether will be found, however long the mixture may be kept. If, after this stage of equilibrium has been reached, the relative proportion of the different substances is altered—if, for example, alcohol be added, or the acid be increased—more ether will begin to be formed; if, on the other hand, alcohol or acid is removed, or water is added, some of the compound ether already formed will begin to be decomposed, until, in either case, equilibrium is once more established.

By means of a formula given by Berthelot, the amount of alcohol contained in the compound ethers of any mixture of alcohols, acids, and water, when etherification is complete, can be calculated, whilst, by a process elsewhere described by the author, the amount actually so present at any time can be estimated. As long, then, as the amount found falls below the amount calculated, etherification is not yet completed; when both agree, equilibrium has been established.

As regards the formation of compound ethers, wine may be looked upon as a mixture of several alcohols and acids with water, and all the foregoing rules will be applicable to it. The estimation of the alcohol contained in the compound ethers of a wine, coupled with the calculated amount, will thus, among other things, give us some information as to the age of the wine. In young wines the amount found will fall below, in older or old wines it will agree with, the calculated amount.

Wines of no very great alcoholic strength have generally arrived at the stage of equilibrium, as regards the formation of compound ethers, in from four to six years; in the case of strong wines, ten years and upwards are required. The conditions under which the wine is kept will, however, greatly influence this time; thus a high temperature promotes, a low temperature checks, the production of ethers.

As previously explained, wine contains both fixed and volatile acids, and these give rise to fixed and volatile compound ethers respectively; that is, such as cannot, and such as can, be dis-

tilled without decomposition. The amount of alcohol contained in the whole number of either class of compound ethers present can be estimated, but the determination of each individual compound ether cannot as yet be accomplished. Fixed ethers have, comparatively speaking, little influence on the character of the wine beyond neutralising part of the acid and facilitating the production of volatile ethers. Among volatile ethers, on the other hand, there are many possessing a very characteristic, and often, especially when much diluted, very agreeable smell; and to them the wine owes a great part of its flavour and bouquet, and probably also some of its exhilarating and stimulating effects.

As regards the proportion existing between the amounts of volatile and fixed ethers present, all pure natural wines, hitherto examined in this respect, contained a greater proportion of volatile than fixed ethers, in spite of the great preponderance of fixed acids; nearly all fortified wines, on the other hand, contained a greater proportion of fixed ethers, although the proportion of volatile acids is generally higher than in the former. Sherries and Madeiras, however, although undoubtedly fortified, form an exception to this rule; they contain, like the natural wines, more volatile than fixed ethers. Lastly, it appears that as the fortified wines get older, the proportion of volatile ethers becomes greater, so that at last they equal, or even exceed, the fixed ethers in amount, the wine gradually coming round to the natural standard.

It seems very desirable that medical men should direct their attention to this difference among wines. By selecting, for example, such wines as contain the highest proportion of volatile ethers relative to their alcoholic strength, it may sometimes be possible to produce the desired stimulating effect with the minimum amount of disturbance often following the administration of alcohol.

The total amount of compound ethers present in any wine is extremely small. Thus in the highest case yet found (a Madeira, which had been 50 years in bottle) the alcohol contained in the compound ethers of the wine (both fixed and volatile) amounted to only one part in 800 parts of wine—equivalent, roughly speaking, to about one part of compound ether in 300 parts of wine.

Glycerine and Succinic Acid.—As previously explained, 107 parts of cane sugar, when submitted to fermentation, yield, according to Pasteur, about 3·6 parts of glycerine and 0·60 parts of succinic acid; all fermented liquids should therefore contain small quantities of these two substances. At present, little is known as to the amounts actually present in different sorts of wine. Succinic acid is probably without much

influence on the character of the wine. Glycerine, however, besides being present in larger quantity (about one fourteenth part of the amount of alcohol) has a well marked sweetish taste, and may account for the like taste possessed by many wines that are perfectly free from sugar. Besides this, it seems to mitigate the harshness of taste belonging to several of the other constituents of the wine, and thus to render it milder than it otherwise would be. On account of these properties, and not being liable to fermentation, it is now used extensively for the adulteration of wine.

Albumenoid Substances.—Grapes contain a not inconsiderable amount of some albumenoid substance which, like all such, is extremely liable to decomposition, and to induce decomposition in other substances in contact with it. Accordingly, when exposed to the action of the air by the crushing of the berry, it absorbs oxygen and yields the ferment under the influence of which the sugar is transformed into carbonic acid and alcohol, whilst the latter may even be converted into acetic acid, the ferment, or yeast, absorbing the albumenoid substance and, by rendering it insoluble, removing it from the wine. In all white wines, properly fermented, this albumenoid substance is almost entirely absent, having been precipitated during fermentation. Such wines are accordingly but little liable to further change. In imperfectly fermented wines, on the other hand, some albumenoid substance remains dissolved, rendering the wine liable to fresh fermentation if not protected by an addition of spirit. All red wines, owing to their having fermented on the husks, contain, when young, much albumenoid substance, which, however, in their case, is preserved from change by the tannin present. In the course of time the greater part of it is thrown down with the colouring matter and tannin.

Mineral Substances, or Ash.—When wine is evaporated, and the dry residue left is heated for some time to a dull red heat, there remains a greater or smaller proportion of mineral constituents, called ash. The nature and quantity of ash left by a wine is one of its most characteristic features. Generally speaking, the total amount of ash ranges in pure natural wines between 1·5 and 2·5 parts per thousand. Under normal conditions this ash consists of carbonate,* sulphate and chloride of potassium, chloride of sodium, phosphate and carbonate of calcium, with traces of silica, magnesia, and iron; frequently there are also minute traces of lithium, and sometimes of manganese. In many southern countries it is the custom to add plaster of Paris to the *must*; one effect of this addition is a great increase

* This carbonate is not contained as such in the wine, but is produced by the decomposition of a salt of potassium with an organic acid, such as the tartaric or malic acid, during ignition.

in the proportion of ash, owing to the production of sulphate of potassium. Sulphate of calcium (plaster of Paris) and cream of tartar give rise to the production of tartate of calcium (insoluble), sulphate of potassium, and sulphuric acid; and in wines so treated the ash rises frequently to five parts and more per thousand, consisting almost entirely of sulphate of potassium. The ash of these wines is frequently free from carbonates and chlorides, because the small quantity of sulphuric acid produced, as above explained, expels the weaker and volatile acids in the course of evaporation and incineration. No wine is met with free from chlorides.

Total dry Residue and Extractives.—If a quantity of wine be carefully evaporated on a water-bath, a greater or less proportion of dry residue is left which contains all those constituents not volatile at 100°C. The greater part of this residue usually consists of sugar, acids, mineral constituents, &c.; but in all genuine wines there is always, in addition, a small proportion of substances the exact nature of which is not at present known, and therefore do not as yet admit of quantitative estimation. Accordingly, if from the total dry residue left all those substances admitting of estimation are subtracted, a small amount will be left unaccounted for. This consists of what are generally termed extractives. No genuine wine is free from these extractives; they may therefore form a valuable feature for distinguishing the true from the factitious article. In wines free from sugar these extractives frequently constitute the greater part of the total residue.

In pure natural wines the total amount of dry residue varies generally between fifteen and thirty parts per thousand. In fortified wines this total residue frequently rises to sixty, eighty, or even more per thousand; it ranges generally between twenty and sixty parts, a great proportion of the residue in these cases being sugar.

THE FERTILISATION OF CERTAIN PLANTS (DIDYNAMIA).

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[PLATE LVI.]

IN the July Number of this Review I described the fertilisation of Salvia and of some other flowers.* I showed how in each case arrangements existed either entirely to prevent self-fecundation, or, at any rate, to render it of rare occurrence. I wish now to show that the same fact is observable in all didynamia, and that the ordinary didynamous structure is indeed nothing more than a contrivance to facilitate intercrossing, and finds in its adaptation to this object its *raison d'être*.

I will begin with a flower abundant enough in our fields, the common red rattle (*Pedicularis sylv.*). This has a labiate corolla, and, as is the general rule in this form of irregularity, the tube furnishes a sweet secretion to attract insects, while the lower lip forms for them a convenient landing-place. The upper lip or hood is remarkably flattened, as though the flower had been pressed in a book. The upper end of this hood is closed for some little distance in front completely, and the stigma projects from it just at the point where the complete closure ceases. Below this again, from the stigma down to the projecting point, marked *a* in the illustration (fig. 1), the hood is practically closed by the close approximation of the two sides, so that the stigma is effectually shut off both above and below from the interior of the hood. Below the projecting point the hood is open in front, but the fissure is very narrow because of the flattening already mentioned. The style rising from the two-celled ovary runs at the back of the corolla in the closest contact with it, following its contour exactly, and thus the curved outline of the closed part of the hood causes the style to bend round, and brings the stigma into the position it occupies. The stamens are four in number, two rather longer

* viz. Mallow, Lopezia, Larkspur.

than the others. Their anthers are lodged in the closed part of the hood, filling completely all the vacant space. They are slightly attached by their external surface to the walls of the hood, while their dehiscence is on their internal surface; so that, if you split open a flower longitudinally, as in fig. 1, you expose the interior of the anther cells on one side to view. The dehiscing surface, therefore, of the anthers of the right side is face to face with the dehiscing surface of the anthers of the left. Now the edges of the open anther cells on one side exactly correspond with the edges of the open anther cells on the other, just as the edges of one valve of an oyster exactly correspond with the edges of the other. It will thus be seen that the pollen grains of the opposite anther cells lie in a common cavity, and so long as the opposing edges of the cells are held in close contact, the pollen is kept securely imprisoned. How is this holding together brought about? As to the upper anthers there is no difficulty, for the part of the hood in which they lie is so excessively flattened as necessarily to press them together. The two lower anthers are in rather a wider space, and might perhaps retreat from each other, and let the pollen escape, were it not for a number of stout hairs underneath them, which by their elastic pressure prevent the separation. These hairs spring from the filament of the upper anther on either side, which passes underneath the lower anther; and it is to be noted that no such hairs grow on the filaments of the lower anthers, where their presence would serve no useful end. The four anthers with the style so completely fill up the closed part of the hood that all is held tightly packed, and it is next to impossible for the parts to get disadjusted.

We have now to consider what is the use of this exquisite workmanship. It is very simple. The flower is visited by large humble bees, which are attracted by the nectar of the tube. As the bee approaches the mouth of the tube, it strikes the projecting stigma with its head, and then settles on the lower lip. It cannot now reach the nectary without inserting its head into the fissure of the hood; but this, owing to the flattening already described, is so narrow that the broad head dilates the hood, and forces the two sides more widely asunder. The necessary result of this can be foreseen. The anthers are attached, as I have said, externally to the inner surface of the hood; consequently, the widening of the hood draws the opposite anther cells apart. A little fissure is formed between the edges, which were before in close approximation, and through this the dusty pollen falls in a little shower on the back of the bee's head. With this adhering to it the bee flies off, and, striking the stigma of some other flower with its pollen-daubed head, fertilises it. I am unable to assert positively that the bee

in its exit from the flower does not touch the stigma, and so fertilise the flower with its own pollen. The bee is so large comparatively, and so abrupt in its departure, that though I watched repeatedly, I was unable positively to assure myself on this point. But it appeared to me that it did not, and I fancy that the projecting point below the stigma serves as a safeguard against this (*a*, fig. 1). At any rate, it is easy to convince oneself by watching that not all the pollen is left on the stigma of the same flower, but that much is carried off to other blossoms. Moreover, when the bee leaves a flower, the stigma is already occupied with the foreign pollen which the insect left there in entering, and there is, consequently, little or no exposure of viscid surface to which the fresh pollen may adhere.

The structure of yellow cow wheat (*Melampyrum*) (fig. 2) so closely resembles that of red rattle that I need not describe it in detail. Here also the opposite anthers form one common receptacle, which is kept closed till a bee opens it by dilating the hood. There are some small differences to suit the arrangement to the somewhat different form and position of the flower, which is set horizontally, and not erect as in red rattle, but the main features are the same.* One little point is, however, worth noticing, and that is that the narrow hood fissure through which the insect must pass its head is made narrower still by the filaments of the front pair of stamens. The insect's head passes between these, pushing them asunder, and thus the dilating force acts still more directly on the opposing anthers than is the case with the other flower.

The cow wheat is fertilised by the small buff-bodied humble bee. The large humble bees also visit the flower, but apparently the fissure is too small for their heads, for they adopt the common treacherous habit of bees when they are unable to get in at the mouth, and make a hole in the tube just above the nectary, and so reach the secretion. The very small calyx admits of this robbery, whereas in red rattle the large leafy calyx acts as a safeguard.

What practised thieves these large bees are, is plain from the fact that, on gathering 100 open blossoms, I found the hole in 96, only 4 being intact. It occurred to me that I might use this habit of the bees to throw light on a point of some little interest, the period, namely, at which a flower begins to secrete its nectar. If there be nectar in the tube before the flower opens,

* In order that the pollen may escape and fall upon the bee, the fissure between the opposing anther cells must occur at their lowest part. This, in the erect *Pedicularis*, is on the side turned towards the tube; in the horizontal cow wheat, on the side turned towards the inferior lip. In each case the fissure occurs where it is required.

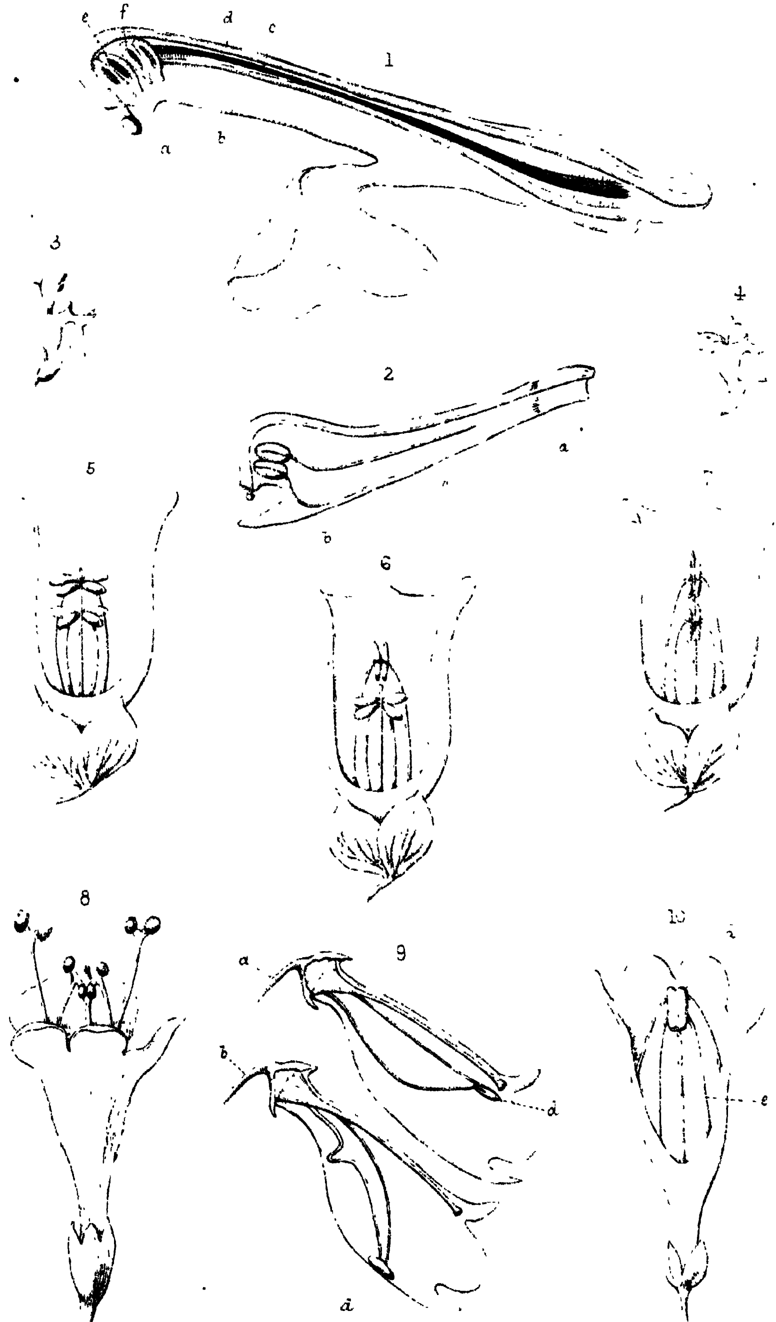
then these marauding bees would not wait for that occurrence, but would make holes in the buds. I therefore gathered 100 flower buds, as nearly open as might be, in the same place and on the same day as had furnished the 100 open blossoms. In 89 of them there was no hole, in 11 only was one present. It seems a fair inference from this, that nectar is not secreted until the flower is on the point of opening. It is plain how accordant this is with the view I have taken of the use of nectar; namely, that it attracts insects to enter by the mouth of the flower.

Among the figworts there are other species nearly allied to the two I have described—bartsia, for instance, and yellow rattle (*Rhinanthus*)—in which there is a very similar arrangement of the anthers and a similar method of fertilisation.

The coherence of the anthers, and the mechanism for fecundation in the species as yet mentioned, may be called exceptional. In most didynamia there is a simpler arrangement, and this must now be considered.

The flower which I shall take as an example—the woodsage, or *Teucrium Scorodonia*—though not very attractive in appearance, presents some points of interest. Figs. 3, 4 will give a sufficient notion of its shape. It will there be seen that the flower differs from most of its tribe in having no hood formed by the upper lip. The stamen and pistil are, however, protected while the flower is in bud by the lower lip, which is so bent back as to cover them in and shut out wind and rain, while it also prevents the premature visits of importunate insects. So soon as the anthers are ripe, this lip turns over and becomes the landing-place of insects. The stigma is at this time immature, and lies behind the stamens out of reach. The stamens, on the other hand, incline forwards (fig. 3), so that the ripe anthers, which dehisce in front, are in such a position that the bees, which visit the flower in abundance, cannot fail to rub the back of their heads against the pollen. So soon as the stigma is ripe, the stamens, which have now shed all or most of their pollen, retire backwards, bending over the upper lip; while the style comes forwards, and occupies the position which before was held by the anthers (fig. 4). It is plain that this arrangement brings about the fertilisation of the older by the younger flowers, an occurrence of which we have numerous examples.

Let me now notice another point. The stamens in this flower are of unequal length. Two are long, two short, or, in botanical language, the flower is didynamous. Can any use be ascribed to this arrangement? I think there can. Were all the stamens of equal length, the hinder pair would be impeded by the front pair, and the pollen of the former would be wasted on the posterior surface of the latter, and never come into contact at all with the bee. This evil is avoided by their longitudinal



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The Fertilization of Plants

arrangement. It may indeed be objected that the evil would have been equally avoided by all four anthers being placed in one horizontal line. Of two perfectly equal arrangements, nature, however, can but select one. But, in fact, the horizontal position would not answer all the requirements. Let any one watch a bee visiting a woodsage. He will see that the anthers strike the top of its head just between the eyes, and smudge the whole intervening space with pollen. Had the two lower anthers been placed in a horizontal line with the upper ones, their pollen would have been shed on the eyes of the insect, and its visits discouraged. The pollen, moreover—and this is the point of chief importance—would have adhered to parts which are quite external to the range of a centrally placed stigma.

If the explanation I have just given be the true one, it should apply not merely to woodsage, but to all those numerous other flowers—labiates, figworts, and the rest—which have a similar arrangement of their stamens. Thus all such didynamous plants must be supposed to require the agency of insects for their due fertilisation. Now that this is really the case is, I think, highly probable. Why otherwise are they, as a rule, furnished with sweet secretions and aromatic odours which serve to attract insects? Why are their four stamens so arranged against the walls of the corolla as to leave a free and open access to the nectary? Why are all the four anthers and the style placed on the same side of the corolla, and in the median line? Why is the dehiscent surface turned towards the path which leads to the nectary and away from the stigma? Why does this latter so often become mature at a later period than the anthers? Why are there only four stamens, even when the corolla has five divisions, and why is the missing stamen invariably the posterior one? Why, lastly, do we so frequently find some or other arrangement in these didynamous flowers, which seems directly calculated to render self-fertilisation a matter, to say the least, of difficulty? All these questions admit of easy answers, on the hypothesis that didynamia are fertilised by insects, and are unanswerable on any other hypothesis.

Let any one, for instance, examine a spike of foxglove, beginning with the upper or less mature flowers, and going gradually downwards to the more mature. He will find, first, flowers in which neither anthers nor stigma are ripe. Then come flowers in which the upper pair of anthers are ripe, the lower pair and stigma still immature. Then all four anthers are found ripe, the stigma still remaining as before. Then, lastly, come flowers in which the bifid stigma is open and bent forwards, but in which the two upper anthers have discharged all their pollen, while the two lower ones have not yet exhausted their store. Now, why is it that the upper anthers are thus before the lower

ones in their development? It cannot, I think, be for any other reason than that they are in closer proximity; in fact, close against the stigma, and that there would be great risk of self-fertilisation were they not to discharge their pollen before the viscid surface was mature; whereas the lower anthers lie at a distance, and are much less dangerous. Were this not the case, there is no reason why the upper should precede the lower. In fact, when a useful purpose can be served, we find the lower pair preceding the upper. This is the case with woundwort (*Stachys*). Here the lower pair ripen first, shed their pollen, and then retire laterally, one to the right, the other to the left. This they do in order to make room for the style to bend forwards, and come into proper position.

In examining the successive blossoms of a spike of foxglove, another fact will be noticed, which, as well as the general didynamous structure, testifies to the importance of a vertical arrangement of the anthers. Each of these has two large lobes, which in the immature blossoms are placed horizontally, so as to cover a considerable breadth of the corolla. Were they to ripen in this position, most of their pollen would be wasted, for it would adhere to the sides of the humble bee, in parts entirely out of reach of the centrally placed stigmas. To avoid this, the lobes, as they ripen, change their direction and become vertical, lying close to the median line. (These successive changes are shown in figs. 5, 6, 7.) Their range is thus made to coincide with that of the stigma; that is to say, a bee entering a freshly opened flower will have its back daubed with pollen in the same median line as will come into contact with the stigma when it visits a more mature blossom.

The imperative necessity of keeping the anthers in the same line with the stigma may perhaps explain the anomalous structure of the stamens in Prunella. In this flower, curious buttresses project from the outer side of the filaments against the inner surface of the wide hood. These serve, I imagine, to fix the anthers more securely in the median line and to prevent any lateral divergence.

There remains yet another point of interest. Why are there only four stamens in these didynamous flowers, even when calyx and corolla have five divisions? and why is the missing stamen invariably the posterior one? The reason is obvious. This stamen, were it present, would occupy the position which is wanted for the style. It is necessary that this shall lie against the wall of the corolla, so as to leave a free passage for insects. It is necessary that it shall lie against the *posterior* wall and in the median line, or its range will not coincide with that of the anthers, and the stigma will not strike the part of the bee on which the pollen is smeared. In fact, there is no other position

but that of the missing stamen in which the style can be placed.

The best thing, therefore, that can happen is that this stamen shall not be developed at all; and this is what does occur in didynamous flowers. The next best thing is that, when developed, it shall get out of the way and make place for the style. This is what occurs in Pentstemon. Here four of the stamens and the style are arranged as in ordinary didynamous flowers. The fifth stamen, however, is present, but makes place for the style, by running from its insertion in the posterior median line right across the tube to the opposite side of the corolla. It is less in the way here than if it occupied its natural position. Still it is perfectly useless, and in some degree an obstruction. It is not therefore to be wondered at that, as a rule, it produces no pollen, and that not very rarely it is altogether absent. The Pentstemon would thus appear to be on its way to become didynamous.

Doubtless there are not a few didynamous flowers the structure of which may seem to be more or less in contradiction with the preceding remarks. The apparent contradictions are, however, I think, always capable of explanation. Some of these exceptions to the ordinary arrangement I will now consider.

Scrofularia.—In this genus the posterior stamen is still the missing one, although the style and other stamens are ranged against the anterior wall. The absence of the fifth stamen cannot therefore in this case be explained by its place being required for the style. It can, however, be accounted for on another ground. The stamen would be perfectly useless, for its anther would strike an insect on the back, while the stigma only comes into contact with the under surface. The anther is absent therefore on grounds of economy. But as its entire disappearance is not required to make room for the style, it is not surprising to find that it is not so completely absent as in other didynamia, but is represented by a scaly staminode of considerable size.

Gesneria.—Here we have a corolla in general form like that of a foxglove. The style also with its stigma occupies a like position as in that flower; that is, it runs in close contact with the posterior wall and in the median line, occupying the place of the absent stamen. The anthers of the four remaining stamens, instead of being arranged in pairs longitudinally, as in foxglove and most didynamia, lie in a horizontal line immediately in front of the stigma. In the case of Teucrium, I said that such an arrangement would not answer the requirements, because the two external anthers would smear their pollen on parts of the bee which would be quite out of

range of the centrally set stigma. In Gesneria, however, this difficulty is overcome. In the first place, the stigma is very broad, so as to cover a large range. Then the anthers are individually very narrow, and, moreover, adhere to each other by their edges, so that the four together form a disk no broader than the stigma, immediately in front of which it lies (figs. 9, *a*, and 10). The dehiscence of these coherent anthers is, as usual, on the anterior face, that is, on the side turned away from the stigma. Thus, a bee entering the corolla soon after its expansion will get a smudge of pollen on the median line of its back. It will not touch the stigma, for this is shielded by the anther-disk. But when the flower has been some time open, a change occurs in the position of the parts. The filaments bend forwards, and carry the disk away from the stigma right across the corolla, until it comes into contact with the anterior lip, where it remains fixed (fig. 9, *b*). It is plain that when a bee visits a flower in this later condition, the same spot on its back, which in the earlier stage came in contact with the anther-disk, will now come in contact with the stigma: for this occupies the same position as did the disk, and is, moreover, of the same breadth. Thus in Gesneria—as in so many other cases—the more mature flowers are fertilised by the pollen of the less mature.

Antirrhinum.—The general arrangement of style and stamens in snapdragon is the same as in foxglove; but the persistent closure of the mouth of the corolla might seem a certain proof that the fertilisation is independent of insects. Any one, however, who watches the flowers with a little patience will soon see that the closure is not sufficient to exclude bees. Sometimes a bee will be seen trying in vain to force an entrance; but in such case the flower is, I believe, invariably immature. So soon as the anthers are open, the tightness of the closure relaxes sufficiently to allow the bees to force their way in without any great exertion, and I have repeatedly seen them do so.

That such visits are required for due fertilisation I have, moreover, found on experiment. I covered a large Antirrhinum with a tent of gauze, so arranged as to exclude bees. The plant flowered abundantly; but though in other Antirrhinums close by, which were not so protected, scarcely a single flower failed to be fertilised, only two small capsules were produced from all the numerous flowers of the protected plant; and even of these two it was doubtful whether the fertilisation was not due to an accidental rent in the gauze that occurred towards the end of the experiment, and was not immediately mended; so that possibly a bee may have got in at that period.

I was much struck in this and a few similar experiments by

the long persistence of the corolla when fertilisation is prevented. I was informed, also, by a large horticulturist, that when the bees get at his flowers in the greenhouses the plants become almost worthless for sale, owing to the speedy fall of the blossom. The teleological significance of this is of course apparent. The corolla is of no further use when fertilisation has once occurred, and its maintenance is a useless burden. It is therefore cast off. But *how* this is brought about is not so obvious. I presume that the fertilisation causes a larger flow of nutriment to the stimulated ovary, and that this flow takes place at the expense of the corolla, which then perishes from starvation.

The last exceptions I shall consider are wild thyme (*T. serpyllum*) and marjoram (*Origanum*).

In each of these we have a didynamous flower, which certainly is fertilised by insects, for its anthers ripen and shed their pollen before the stigma is mature. The four anthers, however, are not placed longitudinally, one pair above the other, for the stamens diverge laterally and project beyond the corolla in a fan-shape. The breadth which the anthers thus cover is much greater than can come within the range of the centrally placed style. The difficulty, however, vanishes when, instead of examining a single flower, we consider the general mode of growth and the inflorescence. Thyme, as a rule, grows in patches. The small flowers are crowded together on the surface of the patch, forming a continuous carpet of bloom, from which project upwards the ripe anthers of the younger and the ripe stigmas of the older flowers. Over this carpet crawl the bees, their bodies so large in proportion to the individual blossoms as to be in contact with many at a time.

The crowded heads of marjoram form a similar carpet. Thus all parts of the under surface and sides of the bees get dusted with pollen, and similarly all parts come sooner or later into contact with stigmas. There is thus no occasion for that definite arrangement of the anthers which is profitable in the large-flowered didynamia. What is required is that the anthers should project beyond the corolla, and this they do.

It would clearly also be disadvantageous that the dehiscent surface of the anthers should be turned, as in most other didynamia, forwards. To ensure the readiest contact with the insect as it crawls over the flowers, this surface should face directly upwards. Now with the longer stamens, which are entirely free of the corolla, this is the case. The lower pair of anthers only, as a rule, half overtop the corolla's lip. The dehiscent side of their upper free lobe is turned upwards; that of the lower lobe faces forwards. Thus always the dehiscent surface assumes the most advantageous position. That of each free lobe is in

the position which brings it into contact with the bee as it crawls over the surface; that of the protected lobe is such that it will strike the bee's head as it sucks the nectar (fig. 8).

In most thyme plants the flowers are hermaphrodite. The stamens project from the corolla when the flower first opens; but it is only at a later period, when the pollen is in great part shed, that the style lengthens, and the stigma in its turn projects. This would seem to afford a tolerable security against self-fertilisation. Nature, however, seems bent on erecting a still stronger barrier, by entirely separating the two sexes. For a considerable proportion of thyme plants have flowers in which the stamens are abortive, and reduced to mere rudimentary points inside the tube, while the stigma projects when the flower first opens.* There are thus thyme plants which bear only female flowers. Whether there are also others which bear only male flowers, I cannot say. I have, however, never found such an one. All that I have been able to make out is that very frequently in the hermaphrodite flowers the stigma fails to arrive at maturity. It seems hardly too rash to foretell that in the course of time these hermaphrodite flowers will cease altogether to possess stigmas, and that thyme will be purely diœcious.

Still more surely may it be foretold of another plant—the horsechestnut—that in time it will be purely monœcious. If one of its pyramidal flower spikes be examined, the greater number of the flowers will be found to have the following structure. A sweet fluid is secreted at the base of the corolla, and access is open to this just under the two upper petals, on each of which is a dab of bright colour, while the rest of the corolla is white. The stamens are so curved and the anthers so set that a bee cannot get at the nectary without smearing its under surface with pollen. In the midst of the stamens a small pistil will be seen, which never developes to maturity, so that these flowers are in fact male flowers. Some few flowers, on the other hand, will be found in the spike, and almost always in the lower part, in which the style and stigma are fully developed, and have just the same curved form and position as above belong to the stamens and anthers. In these pistillate flowers there are also stamens; but these are not turned towards the entrance to the nectary. Their anthers, moreover, as a rule, fall off without dehiscing, although there is pollen to be found on section within their lobes. These lower flowers, then, are practically female flowers, and their stigmas will strike

* The flowers in which the stamens are rudimentary have also a much shorter tube than have the ordinary ones, so short that the lower lip is in contact with and supported by the two anterior long teeth of the calyx.

the same part of a bee which above would be struck by the anthers.

Besides these male and these female flowers, others will be found—usually intermediate in position—which appear to be fairly hermaphrodite. In these not only do the anthers ripen their pollen, but the pistil, though it is not nearly so large as in the purely female flowers below, yet matures and is often fertilised. It is not without its use that the flowers which are becoming male should be at the apex, and those which are becoming female should be at the base of the spike; for thus any pollen which is shaken out by the wind will have a better chance of being utilised than were the positions reversed, as in poterium.

A word as to the dabs of bright colour on the upper petals. It is so excessively common to find such ornaments about the entrance into the nectary of flowers that one can hardly help suspecting that their position in that part serves some useful purpose. May it not be that the colour acts as a guide to the insect, attracting and directing it to the proper entrance? The marvellous ease and rapidity with which insects find their way to the nectary would thus in part be explained. A fact given by Mr. Darwin, as a striking case of correlation, seems to me strongly in favour of the explanation I have advanced. "I have recently observed," says Mr. Darwin, "in some garden pelargoniums that the central flower of the truss often loses the patches of darker colour in the two upper petals, and that when this occurs the adherent nectary is quite aborted; when the colour is absent from only one of the two upper petals, the nectary is only much shortened." *

I should say that in pelargonium, as in a host of other flowers, the anthers shed their pollen before the stigma is mature; and that anthers and stigmas turn when mature upwards, so that a bee, in getting to the nectary, rubs them with the under surface of its body, and thus carries the pollen of the younger flowers to the stigma of the more advanced ones.

EXPLANATION OF PLATE LVI.

FIG. 1. *Pedicularis sylv.* Longitudinal section, (*a*) point, which probably protects the stigma during egress of bee; (*b*) anterior filament, with hairs on its upper part where it passes under the lower anther (*f*) to terminate in the (*e*) upper one; (*c*) posterior filament, without hairs on the upper part; (*d*) style; (*e* and *f*) upper and lower anthers. The cells are open and face the spectator.

* "Origin of Species," p. 145.

FIG. 2. *Melampyrum pratense.* Longitudinal section, (*a*) ring of glandular hairs; (*b*) filament of anterior stamen, on which the bee presses laterally; (*c*) posterior filament.

FIG. 3. *Teucrium scorodonia.* Just expanded. The anthers full of pollen, dehiscent in front, and anterior to the style.

FIG. 4. *Teuc. scor.* At a later period. The anthers empty, thrown back with the filaments; the style inclined forwards.

FIG. 5. *Digitalis purp.* All the anther lobes horizontal and not yet open.

FIG. 6. *Digitalis purp.* The upper anthers with their lobes vertical and dehiscent. The lower anthers still horizontal.

FIG. 7. *Digitalis purp.* The lower anthers also with vertical dehiscent lobes.

FIG. 8. *Origanum vulgare.* The style yet immature.

FIG. 9. *Gesneria sp.?* (*a*) Just open and with anther disk in front of the stigma; (*d*) anther disk. (*b*) Later stage — anther disk (*d*) against anterior wall.

FIG. 10. *Gesneria sp.?* Seen from front, a piece of the corolla having been removed; (*d*) anther disk in front of stigma; (*e*) style, the stigma hidden by the disk.

ON SOME INTERESTING POINTS IN THE HISTORY OF THE POLYZOA.

BY THE REV. THOMAS HINCKS, B.A.

THE *Polyzoa* have recently engaged the attention of many excellent observers and physiologists, and the result has been that much new light has been thrown on their history. The members of the class offer many attractions, both to the collector and the philosophic zoologist, and accordingly they have been diligently sought for and earnestly studied. The number of known forms has been largely increased, and a corresponding advance has been made in the knowledge of their structure and development. But whilst we have been steadily attaining a more accurate conception of what they are, we seem to be as far removed as ever from any general agreement as to what *they should be called*. Continental Europe pertinaciously insists on giving them Ehrenberg's name, *Bryozoa*; England and America are equally unanimous and resolute in their adhesion to J. V. Thompson's very happy designation, *Polyzoa*. It is in all ways desirable that scientific nomenclature should be uniform and cosmopolitan, and it must be accounted a scandal that France, Germany, and England cannot agree upon a name for this important group. If the subject were referred, in the fashion of the day, to a conference, which might take place at the meeting of our own British Association, or of one of the kindred societies on the Continent, it might be possible to get rid of a diversity of usage which represents no scientific principle and has no special significance whatever.

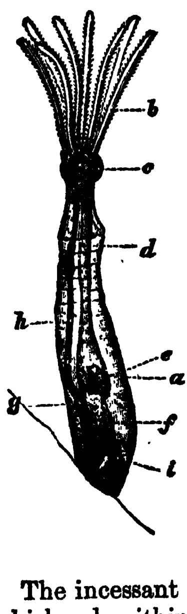
The *Polyzoa* have had their vicissitudes of fortune amidst the revolutions which have swept over zoological science. At a time when external resemblances had more weight with the classifier than the details of organisation, they ranked with the zoophytes. An increasing knowledge of their structure gradually detached them from their Cœlenterate companions, and they appeared first as Molluscan Zoophytes, and then as Molluscoida, at the base of the great Molluscan series. In Professor Huxley's latest work on classification they are raised, in combination with

the *Tunicata* and *Brachiopoda*, to the rank of a sub-kingdom. This, as its author admits, is a merely provisional step; and it is quite possible that, as the result of yet further investigation, they may merge once more in the primary division from which they have been separated. It is but lately that the general plan of the nervous system has been determined, and we are still far from a thorough comprehension of its details.*

It is not my purpose, however, to discuss the systematic position of the group, but simply to notice a few of the more interesting points in its history, and especially to place before the readers of the POPULAR SCIENCE REVIEW some of the most important results of recent research.

And, as a preliminary, it may be useful to give a slight sketch of the form of structure which is characteristic of the *Polyzoa*. In the first place, they are universally composite animals. The primary zooid, the immediate product of the egg, gives origin at once by budding to a second, which remains in organic union with it; and this multiplication by continuous gemmation proceeds on a definite plan, but to an indefinite degree, during the lifetime of the animal. In this way colonies of greater or less extent are formed, in which a multitude of zooids (*polypides*) combine individual independence with subservience to a common life.

Fig. 1.



Each polypide is enclosed in a cell (fig. 1, *a*), which is horny or membranous or calcareous, as the case may be, and is furnished with an aperture, usually placed at one extremity, through which the polypide protrudes at pleasure the anterior portion of its body, bearing a wreath of ciliated tentacles (fig. 1, *b*), in the centre of which is placed the mouth. This aperture is, in some cases, closed by a movable lip, and in others by a much more elaborate operculum. The incessant and vigorous play of the cilia creates a perfect whirlpool within the tentacular crown, by which alimentary particles are borne through the central mouth into a wide

* It is worthy of remark, in this connection, that the important discovery of a "colonial nervous-system," uniting and bringing into relation the many zooids composing a polyzoan community, which we owe to Fritz Müller, and of which we shall have more to say hereafter, is barely referred to by Huxley in his recent work, and is dismissed with the remark that it requires confirmation.

pharyngeal chamber (fig. 1, *c*). From this, by powerful contractions, they are shot down the œsophageal tube (fig. 1, *d*), either into a gizzard (fig. 1, *e*), where they are triturated, or, in the absence of this organ, directly into the stomach itself (fig. 1, *f*), a bag or sac, which is usually of a rich reddish-brown colour, and, with the rest of the viscera, is suspended in the cavity of the cell. Here they are rapidly digested. The unuseable portions are sifted out and borne by the action of cilia towards the upper end of the stomach, which has the appearance of being doubled upon itself; and, after rotating here for a time, they pass through a valvular orifice (fig. 1, *g*) into the intestine (fig. 1, *h*), a long and slender tube which is continued upwards to an outlet at the base of the tentacles. The space that intervenes between the body of the polypide and the walls of the containing cell (the *perivisceral cavity*) is filled with fluid, into which the products of digestion find their way, it is supposed, through the walls of the stomach, and are thus made available for the nutrition of the common life. In this cavity the reproductive organs are developed at certain seasons of the year.

The polypides are lively and rapid in their movements; and a most interesting sight it is to see them issuing from their little dwellings, and suddenly expanding their exquisite bells of tentacles, clothed with a thousand vibrating cilia, and then on the slightest alarm darting back again with the speed of light. A well-developed muscular system supplies the means of all this activity.

A simple nervous system has long been recognised in each polypide, consisting of a single ganglion placed between the œsophagus and the rectum, from which filaments pass off in various directions, and especially, it would seem, towards the tentacular region.* Such is the general plan of structure.

It is impossible to frame any general description of the aspect and habit of the *Polyzoa*. These are as various as the modes of gemmation. They grow in plant-like tufts, stem and branches composed of series of cells linked together; they spread like lace-work, or as fairy chains, over other bodies; they rise into stony coral-like masses; they cover the fronds of the seaweed with fleshy crusts or silvery network; they form colonies of exquisite little frosted tubes. In many of the stony kinds the cells are richly sculptured, the single genus *Lepralia* exhibiting an immense variety of microscopic ornament.

* Nitsche, in an able and interesting paper on the fresh-water Polyzoa, has described and figured an ample and most effective service of nerves in the lophophore of the well-known *Alcyonella*.

Such is the class in general character. I proceed to notice the points of special interest to which I referred at first.

The increase of the polyzoan community by external budding is one of the most familiar facts in its history; but it is only lately that we have learnt that a provision exists for the renewal of the polypides, and that as one generation of them disappears its place is soon filled by another, which occupies the same cells and perpetuates the life of the colony. In the Hydroid Zoophyte the life of the individual polypites is evanescent; they soon perish, and are followed by others which bud from the common flesh. Amongst the *Polyzoa* also it appears that a succession of zooids may occupy the same colony, but the mode in which the second generation is supplied differs widely from that which obtains amongst the *Hydrozoa.* Students of the former class have long been familiar with certain dark-reddish bodies, of somewhat spherical form, which are commonly present in the cells, and remain in them after the death and disappearance of the polypides. Not unfrequently whole colonies of *Bowerbankia* for instance are found, in which almost every cell is tenantless, but exhibits a single dark spot.

These bodies have been commonly regarded as ova, which are not liberated till after the death of the polypide; but they have, as we shall see, a totally different significance, the discovery of which has opened to us one of the most deeply interesting chapters in the polyzoan life-history.* Let us first trace them to their origin, and then enquire into their function. At certain seasons a very marked change is seen to be taking place towards the base of the body of the polypide (fig. 1, *i*). This change consists in the gradual separation of the lowest portion of the stomach from the rest of that organ. After a time a somewhat spherical dark-reddish mass is found to be, as it were, suspended below the stomach, with which it is still connected by a narrow channel. In this semi-detached portion the characteristic contraction of the walls, by which the food is driven upwards within the digestive cavity, may yet be noticed. At this stage there is no absolute separation, but merely a contraction of the stomach, by which its lowest portion is formed into a distinct chamber. At length, but whether before or after the death of the polypide I am unable to say, this inferior section of the digestive sac is completely cut off from the rest and lies within the cell as a separate structure. In this condition it is found to consist of a granular mass enveloped in a membrane, which is thickly covered

* The interesting facts which I am about to detail were first fully brought to light by the Swedish naturalist Smitt, in a very able paper "on the Development of the Marine Bryozoa." His observations have been confirmed by Nitsche; and I have myself verified them again and again.

with pigment spots; and it is now "the dark body" so long known to the student of the *Polyzoa*, and, as it proves, so falsely interpreted. In the species which I have most carefully investigated it remains attached to the cord that connects the polypide with the base of its cell; it occupies indeed much the same position as it did when an integral part of the digestive system. The original tenant of the cell having run its course and disappeared, the bud from its own substance which it has left behind it enters upon a course of development resulting in the formation of a new polypide. It first increases in size at the expense of a mass of fatty globules, by which it is more or less surrounded; then a small bud makes its appearance on its upper surface, which is gradually moulded into the rudiments of an alimentary canal and wreath of tentacles. Development proceeds step by step, until at last a fully formed zooid has been evolved out of the remnant of its predecessor, and the cell is once more in possession of a tenant.

How often this process may be repeated we have at present no means of judging; but the curious provision which I have just described for supplying the colony with at least a second generation of polypides seems to be universal amongst the marine *Polyzoa*.*

I have already referred briefly to the simple nervous system which has long been recognised in the individual polypides. Recent investigations have shown the existence of a "colonial nervous system" besides, by which the many zooids in a community are united and brought into relation. The original observations of Fritz Müller have received confirmation from Smitt; and I have also been able to trace the common nervous system in a considerable number of species. In the *Polyzoa*, such as *Bowerbankia* or *Valkeria* (fig. 1), in which the cells are set singly or in clusters on a distinct stem which is horny and more or less transparent, it is readily detected. This is not the place for details, but it may be stated generally that a nervous trunk pervades the stem and branches, connecting a series of ganglia which are situated at the joints, and communicating, by means of nervous filaments that sometimes originate in a complicated plexus, with other ganglia at the base of the individual cells. We have seen this structure beautifully displayed in a branch of *Valkeria pustulosa* preserved in fluid, within which a rich nervous plexus could be distinctly observed, giving origin to a multitude of most delicate threads that passed from it to the numerous cells composing the neighbouring cluster. In the calcareous species this colonial nervous system is more

* The whole process may be regarded as a mode of gemmation. Smitt names the detached bud the "groddkapsel."

difficult of detection; but it has already been recognised in several of them, and is no doubt present in all. Without attempting to describe its plan in detail, I may mention that I have traced the main nerve-trunk in *Bicellaria ciliata*, and have observed that a branch passes from it to the base of the avicularium, which is furnished, as we know, with a special apparatus of muscles. The interest and importance of these discoveries will be at once recognised.*

We turn now to the reproductive history of the *Polyzoa*, which offers a few points of special interest. It has long been well known that at certain times both eggs and spermatozoa are developed in each cell, the former from its membranous lining and usually the upper portion of it, the latter towards the bottom of the cell. At a certain stage of their growth the ova escape from the delicate envelope that has hitherto confined them, and lie free in the perivisceral cavity, where they are fertilised and enter upon the course of development that results in their conversion into active ciliated embryos. Smitt, however, has noticed another mode of reproduction, which he regards as asexual. He describes eggs as occurring within the cell (of *Lepralia Peachii*), and lying in a loose mass of fatty globules, by which they are nourished. In proportion as the ovum increases in size this mass diminishes around it, until at last it lies free in the cavity. At this stage it is furnished with a membrane and is of a dark red colour. It afterwards undergoes segmentation, and passes into the condition of a ciliated embryo; but no spermatozoa have ever been detected in the cells in which such ova are developed. The observation is a very interesting one, and to a certain extent I can confirm it. But it is to be remarked that the spermatozoa are difficult to detect, and we require the evidence of repeated observation to assure us of their non-existence. Smitt has recorded the occurrence of this asexual mode of propagation in several species, and names it "the formation of ova by internal budding."

The structure of the curious embryo, which is the immediate product of the egg, has been thoroughly investigated by Nitsche and illustrated by admirable figures.† It exhibits some very interesting points. The shape of the embryo varies in different species, and is often difficult to describe, but the plan of structure seems to be pretty uniform throughout the class. The whole surface of the body is covered thickly with cilia, which exhibit some diversities of size and form, and by means of which it swims rapidly through the water. Its movements are

* Reichert has challenged Müller's conclusions; but his criticism seems to be based on the examination of a single species, and is not sustained by the results of further investigation.

† *Beiträge zur Kenntniss der Bryozoen.* Von Dr. Heinrich Nitsche. 1869.

lively and varied: now it sails swiftly along, tumbling over and over occasionally in a rollicking fashion; now it rotates perseveringly on its axis; now it moves restlessly over the surface of the vessel in which it is confined, as if searching for something. The decline of its activity is the sign that the first stage of its existence is drawing to a close.

The cilia are borne on a delicate membranous envelope, within which the solid contents of the body are seen as an opaque white mass. At one pole of the body is placed a somewhat horseshoe-shaped opening, at the extremity of which is a tuft of long bristles that lash the water vehemently and incessantly. This opening is without doubt a mouth; I have seen *rejectamenta* passing from it. At the opposite pole is a circular depression, forming a kind of bowl, the margin of which is surrounded by very delicate cilia. This, as Nitsche conjectures, is very probably a sort of sucker by means of which the embryo attaches itself. I have little doubt that this is the true interpretation of it, as I have noticed that this portion of the body is always applied to the surface on which it rests. In some species minute red pigment spots are present at certain points on the surface of the body, in which a refractile corpuscle is embedded. Are these to be regarded as the equivalent of eyes?

When its term of free life is ended—that is, when it has attained the proper stage of development—the embryo fixes itself, loses its cilia and the whole of its characteristic structure, and now appears as a membranous chamber inclosing a mass of granular substance; and from this mass the polypide gradually buds. The foundation of the colony is thus laid.

A word in conclusion respecting the curious appendages with which so many of the calcareous *Polyzoa* are furnished, and which bear the names of *vibracula* and *avicularia*.* The former are long and slender bristles placed close to the cells, which move incessantly, with much energy and a certain rhythmical sweep, over the surface of the polyzoary. They are, as it were, jointed at the base and supplied with a special apparatus of muscles, and their function plainly is to prevent the accumulation of noxious matter in the neighbourhood of the polypides. The *avicularia* are developed in extraordinary profusion and variety throughout a large portion of the class. They are best described as miniature birds'-heads, and are furnished with a movable lower jaw which they keep in constant play, now throwing it back with ludicrous vehemence, and after a while closing it with a malicious snap. Many of them are fixed,

* Both these organs may be studied to great advantage in one of the commonest of our British species, the *Scrupocellaria scruposa*.

and their action is confined to an incessant exercise of the lower jaw; but in some cases they are mounted on a kind of joint, on which they swing backwards and forwards in a half-rhythmical way, opening and closing their mouths with prodigious vigour.

I am not aware that any recent observations have been made tending to throw light on the relation which these curious organs bear to the economy of the Polyzoan. The capture by the *avicularia* of small worms, which are much given to wriggling in and out amongst the branches of the colony, has often been observed; and I have been surprised to find an unhappy captive of this kind still in durance and still living after the lapse of two or three days. The birds'-heads may also be seen frequently with a quantity of dirt between their beaks, as if their office too might belong to the scavenging department. I have referred to them, however, for the purpose of noticing the familiar fact, that they continue their movements after the disappearance of the polypides, and may often be seen in full play when there is no sign of life in the rest of the colony. The explanation is that the common nervous system, on which they are dependent, no doubt retains its energy for some time after the death of the polypides; and, further, that in a large number of such cases, though the cells are tenantless for the time, the life of the colony is not really impaired, but a company of the "*Groddkapseln*" is on the point of supplying it with a new population.

REVIEWS.

THE UNIVERSE.*

THIS is the epoch of luxurious scientific publications, and this year is almost its acme. Never, within our recollection, have we seen so many editions *en luxe* of scientific treatises as the past twelve months have produced, and to conceive of anything which can in future years surpass in magnificence those of 1869 would be simply impossible. It is in no ecstatic spirit that we employ these words. Exaggerated as the praise may seem, it is, in fact, but a "plain unvarnished" statement of the extravagance in mechanical execution to which English publishers have recently gone. In one sense the circumstance is to be rejoiced in, as it shows that enterprise receives encouragement, but in another sense it is to be regretted, for it tells us the too apparent truth, that the popular scientific works, with elaborately gilt covers and illustrations which are artistically excessive, are the necessary bait to induce the British public even to nibble at the hook which philosophy throws out to capture superstition.

Of all the exquisitely "got up" books that have come under our notice, this work of M. Pouchet's, which Messrs. Blackie have issued, is, in its English garb, the most splendid. Whether we look to the type, the paper, the binding, or the multitude and quality of the engravings, we are still bound to confess that in these respects it cannot be surpassed. This is more than can be said even for most of the books of this class, for when we come to the "matter" they contain, we generally observe a quantity of garbled facts ill assorted, badly conveyed, often at variance with each other, and frequently misunderstood by the reckless compiler, who has written the book "to order." This expression of opinion would apply to a very considerable number of the popular scientific treatises which have of late been imported into this country from France. But let us clearly state that it would not at all apply to the present work, which is really a sterling book, notwithstanding that it attempts an account of all the natural objects which surround us. We fear that a great many persons—even of those who think themselves educated—will be disposed, from the external features of this book, to rank it with M. Figuier's successful *réchauffées*. If this should occur, it will be a great mistake, and one much to be regretted. We have

* "The Universe; or, the Infinitely Great and the Infinitely Little." By F. A. Pouchet, M.D., Corresponding Member of the Institute of France. Translated from the French. London: Blackie and Son, 1870.

a very good opinion of M. Figuier and his compilations; but to compare him with Pouchet, or to class the books of the two in the same category, would be a monstrous injustice to the latter. M. Pouchet has his faults; he is given occasionally to confounding inferences with facts, and to dogmatising a little too strongly on points in heterogeny; but, whatever his sins, he is not to be regarded as a scientific quilldriver, but as an earnest and industrious investigator in some of the most difficult paths of science. We trust, then, that readers will understand that, in writing a popular treatise, M. Pouchet is coming down from his usual pedestal of research to do a good work—to lend a hand conscientiously to the furtherance of a knowledge of science among the people.

M. Pouchet's book, then, is just such an one as might be expected of its author. In accuracy it leaves nothing to be desired. Its scope is immense, and therefore the method of dealing with the several subjects is of a general kind, details being, as far as possible, avoided; but it is not on this account sketchy. If the colours are laid on roughly with the knife, there has been a careful study of the *tout ensemble*, the effect is Turneresque, and the whole picture is one—not pre-Raphaelitish, showing us a myriad of small things, but—which gives us a fine broad landscape in which the universe itself is depicted.

And what does this book tell us of? asks the enquiring reader. In answer, let us say that it treats upon all, or nearly all, natural phenomena, and some unnatural ones. The leading facts in zoology, from the polycystin to the elephant; in botany, from the bacterium to the palm; and in geology and palæontology, from the simplest elementary fact in ordinary denudation to the consolidation of a glacier or the elevation of a mountain range, from the foraminifera of the chalk to the huge Dinornis or Megatherium, all find a place in M. Pouchet's history. And on all he tells us if not something new, at least something put in a new fashion, and in forcible, vivid language, and in a way which gives us something besides the fact—gives us an idea of its relation to other facts, and suggests something additional in the way of generalisation. The style is fresh and vigorous, like that of a preacher whose heart is in his sermon and whose congregation doesn't slumber. In short, the author carries his reader along with him. All sections of the work are good, but we fancy the entomological portion is a little too extended, not absolutely so, but in relation to the other departments. The author's notes are numerous and carefully prepared, and are well supplemented by those of the editor, who, we may remark, has drawn largely on our pages for material.

The portion of the work to which our interest attaches most largely is the last, in which M. Pouchet gives a popular sketch of the arguments for and against heterogeny or spontaneous generation. The critics have overlooked this—a fact not surprising when we mention that the chapter opens on page 712, and that some reviewers believe, with Sidney Smith, that impartiality cannot be exhibited if the critic extends his reading beyond the title page. Here, however, is to be found the best outline of this important controversy which has yet been given. We exclude M. Pennetier's book because it is in French. In this chapter the English general reader is first initiated into the important discoveries of Pouchet, Musset, and others, and

we believe that the result of his study of it will be his nearly entire conviction of the inadequateness of the theory of Panspermy. But whatever he may think of this part of the book, he cannot fail to be pleased with the work as a whole, and he will, we doubt not, appreciate the remarkably good English of the translator, and the very great enterprise of the eminent publishers who have introduced the book into Great Britain.

THE SUN.*

OF all the discoveries which have been made during the present century in the department of astronomical physics, very few are more remarkable than those which have been made concerning the sun. The solar spots and the solar prominences have, during the last ten years or so, occupied the attention of European and American astronomers, and the hypotheses which have been started in explanation of these phenomena have been as numerous as conflicting. It is not at all surprising, therefore, that a work should be published upon the solar phenomena as a whole; indeed, the only thing to be wondered at is that such a work should have made its appearance in France and not in England, where the labours of De la Rue, Huggins, Lœwy, Stewart, Lockyer, and Stoney have done so much to advance our real knowledge of the constitution of the sun. M. Guillemin, the author of the work which Dr. Phipson—a former contributor to these pages—has introduced to the English public, is an old friend of English popular astronomers. His work on "The Heavens," translated some time ago by Mr. Lockyer, is familiar to most educated persons, and, as it deserves to be, is a favourite. The work on the sun will, we trust, be equally well received, for it is an excellent summary of what has been discovered of the nature and motions of the sun from the earliest times to the remarkable spectroscopic researches which, in 1868, revealed to us the true character of the solar prominences. M. Guillemin has had a vast series of facts to collect and describe, and he has discharged the task taken up by him conscientiously and successfully, producing not merely a dry record of the history of astronomical research, but a most seductive and forcible sketch of the progress of solar science. The book is well illustrated and well written, and he must be a dull man indeed who cannot find in its pages much, not only to interest and instruct him, in the ordinary sense of the word, but also to lift his mind above the mere commonplaces of life to the contemplation of that grandest of all great secrets, the cause and end of celestial phenomena. In his labour of translator Dr. Phipson has performed his duty creditably. The book reads like a work written in English, and not simply rendered from another tongue. We have not space to discuss any of the problems treated upon by the author; but we would especially direct attention to his analysis of the different theories—of Kirchhoff, Faye, and others—as to the constitution of the photosphere. The judicial manner in which he contrasts all without leaning

* "The Sun." By Amédée Guillemin. From the French by T. L. Phipson, Ph.D. London: Bentley, 1870.

to any, cannot be too much admired by those who have any knowledge of the tendency which some scientific men have to frame generalisations—and swear by them.

ELEMENTARY MECHANICS.*

MESSRS. GROOMBRIDGE have lately commenced the publication of a series of scientific manuals for university students and men in the higher schools, which, so far as we have seen of them, surpass in comprehensiveness, simplicity and cheapness those of other series as yet issued. The volume on mechanics is no exception to the rule, but is a very carefully prepared introduction to the principles of statics and dynamics, by a gentleman who, himself a medallist in natural philosophy of the University of London, understands distinctly what it is the student requires, and has sought to make his path through the "Matriculation" and "Preliminary" as easy a one as possible. The plan of giving a number of exercises for the pupil to work out himself is a very good one, as is the method of stating the several mechanical facts in a set of definite propositions. The publishers have done their part as well as the author, and with clear, easily read type, good woodcuts, and cut edges, have left the student nothing to desire.

CHEMICAL NOTES.†

DR. ODLING'S object in issuing this volume has been to put before the student the notes or headings of subjects upon which he himself has been in the habit of lecturing. Whether the idea be a happy one we shall not wait to enquire. For the lecturer the book will be most useful, since in the briefest possible space it gives him those facts which he should expand and expound in his discourse to his pupils. It need not be said that the work contains everything which the ordinary student should know, as of course the author's reputation is sufficient guarantee for that. But we must confess that, save for reference, the ordinary student is not likely to favour this work, from the fact that its contents are of so "cut-and-dry" a character. Any sentence in the work might stand by itself as a sort of chemical aphorism, having indeed no grammatical relation to its neighbours. This renders the book very useful to the lecturer or to the working chemist, who wants to hit upon a particular statement with the least possible trouble; but it is a quality which will ever prevent this otherwise excellent treatise from becoming popular with class-men. Wherever we have dipped into its pages the same idea occurs to us, i.e. that the book is a huge "syllabus of lectures," and therefore generally unreadable. Take the following paragraph for example:—"Alkali-silicates with variable excess of silica, known

* "An Elementary Course of Theoretical and Applied Mechanics." By Richard Wormell, M.A., B.Sc. London: Groombridge, 1869.

† "Outlines of Chemistry, or Brief Notes on Chemical Facts." By William Odling, M.B., F.R.S., Fullerian Professor of Chemistry at the Royal Institution. London: Longmans, 1870.

as water glass. Compounds made by fusing excess of silica, usually sand, with alkali-carbonate and a little charcoal to facilitate reaction, &c. Proportions taken variable, ordinarily such as to yield compounds $K_2O.4SiO_2$ and $Na_2O.4SiO_2$. Resulting fused mass having appearance of ordinary glass, powdered and then boiled for some hours in water, until completely dissolved." We need quote no further. Of this unconnected character is the whole work; and while, as we have said, it is invaluable for its condensation of a huge mass of facts, its conciseness renders it a very unreadable work.

DREAMS.*

IF the scientific portion of this volume does not exhibit that acquaintance with the philosophy of the subject which a modern student of psychology should possess, the literary portion, which is clipped from almost every writer whose works are extant, will compensate most people for this defect. Mr. Seafield has conceived the idea of forming a sort of common-place book of everything that has been said and written in regard to dreams, and whatever we may think of the value of such a compilation—and our opinion is certainly not a high one—the general public like a work of this kind, as is proved by the circumstance that the volume now issued is one of a second edition. Physiology is not even yet prepared to explain the phenomena of dreams thoroughly, for the very simple reason that it does not yet understand even the simplest action of the brain. It is useless for us, then, to criticise the notions of Sir H. Holland, and a few others, whose ideas are given by Mr. Seafield to his readers. The fact is, that such notions are purely speculative. They are hypotheses which, though they may give an explanation of dreams, do not necessarily convey the real explanation, but like a host of other possible hypotheses, meet the leading facts. Just in the same way might we start a host of speculations to account for the commission of a dreadful murder. But justice would not be satisfied with mere possibilities. So of Mr. Seafield's book, in so far as its science is concerned. Of its literary merits, there is not much to be said. Most of the work is quotation from well-known authors, ancient and modern. The compiler does not thrust himself forward very often, and when he does appear, we wish him off the stage again as soon as possible. To the philosophy of the question he adds nothing, and his style is painfully characterised by mannerism and affectation, which he mistakes for fine writing. On the whole, the book is not a satisfactory one, viewed from our stand-point.

PHYSICAL SCENERY FROM A GEOLOGICAL ASPECT.†

SOME men ride their hobby a little too fast, so that they are unable to see what lies around them, and are utterly lost in their own way of looking at things. Such an one is the author of this book. Mr. Mackintosh

* "The Literature and Curiosities of Dreams." By Frank Seafield, M.A. 2nd edition. London: Lockwood, 1869.

† "The Scenery of England and Wales, its Character and Origin." By D. Mackintosh, F.G.S. London: Longmans, 1869.

can see in the causes which have produced the scenery of Wales nothing but denudation, as those who know his various papers in the *Geological Magazine* might have anticipated. For ourselves, we believe very fully in the *medio tutissimus ibis* principle, and if Mr. Mackintosh would just assume what our Gallic neighbours call the *juste milieu*, we believe he would soon have more disciples than he can reckon on at present, and would do more service to physical geology. We have no desire in the world to underrate the very great influence of denudation in modifying the physical contour of a country, but to ascribe the scenery of England and Wales to denudation alone is really carrying on the matter too far. There are, we think, very sound geological reasons for believing that before denudation could have affected the scenery in any but the most trifling manner, that scenery was even grander than it is to-day. If Mr. Mackintosh merely means to tell us that denudation has done a great deal, he is but repeating a well-known geological truism. If he intends to prove that it has done all—and we think he does—he is overstepping the limits of legitimate reasoning. His book is a very interesting one, both to general readers and geologists, and it is illustrated by some very pretty sketches from the author's pencil. But for all this, we cannot help regarding it as a peg on which the author has found it convenient and useful to hang out to air those theories of his which have from time to time been so severely handled in the magazine in which they first appeared.

A NATURALIST'S RAMBLES.*

HAVING been in the habit, while walking through the country lanes with his children, of pointing out to them the most striking facts in rural natural history, the Rev. W. Houghton deemed that it would be useful to jot down on paper his conversations with his "little ones." We think he was right, and we believe that his efforts will do much service in improving the minds of children, in cultivating an early taste for scientific study, and in developing original observations in young folk. There is, of course, nothing in this little volume of Mr. Houghton's which has not appeared in a thousand forms in other treatises of a similar character; but the facts are woven together in a novel and natural manner, just as they might be by an intelligent father who sought to instil a few elementary scientific facts into the minds of his children. Truthful and simple, garnished here and there with a telling anecdote, full of detail and yet put in such a way that the little power of concentration which a child possesses is certain to be exercised, the author's dialogues during his rambles may be read to all young people with interest and profit. They are devoid of that painful tendency to hyper-moralisation which is such a bore to the young, and which makes the old look back on their young days, when books of the "moralising school" existed *de trop*, somewhat as we look back on the jam which was used to conceal that dreadful "powder" of our childhood. Mr. Houghton's book is free from these defects, and yet is well calculated to inspire the readers

* "Country Walks of a Naturalist with his Children." By the Rev. W. Houghton, M.A., F.L.S. London: Groombridge, 1869.

with reverence for and admiration of the works of nature. It is illustrated with woodcuts, which are very good, and with chromo-lithographs, some of which are "dreadful daubs," and should have been omitted.

SCHOOL CHEMISTRY.*

SURELY the subject of chemistry is overdone. It would, we think, occupy a whole page of our space to give merely the titles of the students' manuals which have been published during the last three years. Where all the students are supposed to come from, or how all the manuals are supposed to pay, we have not the most remote idea. If our judgment be correct, there are at least half a dozen chemical text books too many. And yet we cannot help admitting that the great majority of these books are really good, carefully prepared, and useful compilations. In the little volume now on our table we have a very excellent epitome of modern chemistry, which, if we may use the expression, is a good deal of the Williamsonian school. It is, we think, to be regretted that the author should have omitted the subject of organic chemistry, and that he should have referred to the atomic theory as an almost established principle, instead of being, as it is, a mere ingenious hypothesis which is contrary to all our metaphysical laws. His plan of giving a series of questions at the end of each chapter is a good one. The list of apparatus required by the beginner, the account of the metric system, and the description of the several crystalline groups are all excellent in their way.

THE MIDNIGHT SKY.†

THIS earth of ours is sweeping round with terrible velocity on its own axis, and it is sweeping round the sun, and the sun and all the planets are probably sweeping away through space, whither is beyond our ken. Are not these strange facts of themselves alone sufficient to awake even in the minds of the most thoughtless some desire to learn even a little of those wondrous heavenly bodies of which we know so much, and of which we have yet to discover such an immense amount? And yet how few among us know even the chief of the constellations, and how very few, even among tolerably intelligent people, there are who could, if they were asked, give a satisfactory explanation of the "precession of the equinoxes"? This state of things should exist no longer, especially while such excellent and attractive works as that which the Religious Tract Society has published for Mr. Dunkin are within our reach. In this volume even those who know

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* "Chemistry for Schools, an Introduction to the Practical Study of Chemistry." By C. Houghton Gill, Assistant Examiner in Chemistry in the University of London. London: Walton, 1869.

† "The Midnight Sky; or, Familiar Notes on the Stars and Planets." By Edwin Dunkin, F.R.A.S. London: The Religious Tract Society, 1869.

nothing of elementary mathematics can learn a great deal of astronomy; enough, indeed, to set up as original workers, should they possess one of Mr. Browning's popular silvered-mirror telescopes. "The Midnight Sky" gives us an account not merely of the groups of stars which fill the firmament, but also of the constitution of the sun, the planets large and small, the cometary bodies, the nebulæ and those curious things the meteors, which a few years ago were looked on as mere erratic cosmical matter, but which, thanks to the labours of Proctor and others, we know now to form a huge system which has its own definite periods of revolution, and which may perhaps one day consolidate into a sphere. Indeed Mr. Dunkin's book is a general treatise on astronomy; but it differs from the works now familiar to readers, by the possession of a multitude of beautiful engravings, so that he "who runs may read," or if he is too careless to read, may learn a great deal from the mere pictures themselves, these being so numerous that, as Dickens says of a certain house with many gables, there are more than a lazy man might care to count.

The most original feature in the work is the plan on which the star-maps are constructed. These are, as it were, little midnight photographs of the sky. Fancy it possible, some of these bright, cold, clear nights, to go up in a balloon with Mr. Coxwell, and then to take a photograph of the sky above, and of London beneath, and you have some notion of Mr. Dunkin's pictures of the heavens. But you should do more than this, you should go up and perform the operation every month, and do it both here and in the southern hemisphere in order to obtain all the handsome views he has given us of the sky. This, however, does not sum the contents. The author has figured and given the history separately of the leading celestial bodies and their phenomena, and with this he has filled that part of his book not already taken up by the midnight sky proper. In style, the author's descriptions leave little to be desired, and in the general tone of his teaching we have no fault to find save when in the end of the book—it is only in the end, and it is a special chapter which may be skipped—he has given us a little of that Bridgewater writing which has done so much to make really thoughtful men become irreligious. Of course this is not its tendency, but most assuredly it is its inevitable result.

MILLER'S ORGANIC CHEMISTRY.*

"Breathes there a man" who has ever entered a laboratory and yet has failed to recognise the sterling value of Dr. W. Allen Miller's great three-volumed, exquisitely-printed and carefully-annotated treatise on chemistry? If there does, then all we can say is that we blush for his ignorance and pity him for his misfortune. Dr. Miller's work, large though it be, is, we feel assured, the student's best friend in chemistry, and its author takes care that it

* "Elements of Chemistry, Theoretical and Practical." By William Allen Miller, M.D., D.C.L., V.P.R.S. 4th edition, Part III. Organic Chemistry. London: Longmans, 1869.

never lags behind the advance of the science which it represents, for hardly a year goes by in which some one of its volumes does not come out in a new edition with revisions and additions. We have been for some time looking forward to the appearance of the fourth edition of the volume on organic chemistry, and just as we went "to press" with our last number it was issued by Messrs. Longmans. It is unnecessary for us to say a word as to the contents, for the chief features of the work are already familiar to chemical readers. We may point out the fact, however, that the present edition possesses certain distinguishing peculiarities. The metric system has been introduced; in the thermometric scales we find the centigrade graduation as well as that of Fahrenheit, and the new form of notation has been adopted. These are the chief points in which the fourth differs from the preceding edition of this excellent work. In other respects, it is simply a book which, above all others, gives us a graphic and readable account of those manifold and yet complex transformations of a few elements which constitute the branch of study called organic chemistry. But it is not simply a book for the chemist; it is a treatise which the manufacturer, the physiologist, and the medical man will not consult in vain.

REPTILES AND BIRDS.*

IF from no other reason, this book of M. Figuier's is useful from the circumstance that recent attention has been directed to the close anatomical relation between reptiles and birds. It is hard to conceive, at first, that the most sluggish—at least some of them—and the most active of vertebrates can have any kinship, and yet the teachings of comparative anatomy, as unfolded by the researches of Huxley and Wagner, leave us no doubt on the point. It is from this circumstance alone that the book before us deserves commendation. That it is a handsome volume, that its illustrations are many and good, that its statements, so far as they go, are truthful, we are bound to confess. Perhaps the reader will ask what more we require? and to this we will reply that these qualities are not all that is wanted to make a book commendable. Much more is requisite. What would a museum be worth in which there was simply a collection? What likewise is the value of a work in which facts alone are recorded? We answer, for the purposes of education, nothing. Now, in this book of M. Figuier's we note the tendency we have found in all his works, and which we have in this country been almost alone in condemning—the tendency to popularise unconscientiously. We hope he will excuse us the comparison, but in reading his multitudinous compilations, we feel that we are learning biology much as we should expect to learn history from some of those stentorian warders who daily take their dozen visitors through the chambers of the Tower of London. There is an uncomfortable "cut and dry" sort of feeling in reading his books, a sort of scissors-and-pasty feeling which we have never yet got

* "Reptiles and Birds, a Popular Account of their various Orders." By Louis Figuier. Edited by Parker Gillmore. London: Chapman and Hall, 1870.

over, and a severe relapse of which came on on reading the present publication. If, as we have said, there is any one point more than another of interest in relation to birds and reptiles, it is the singular affinites in structure which Huxley has lately pointed out, and in our eyes a book which merely refers casually to these as statements "of ingenious men who merely strain facts to support a preconceived hypothesis," is one much to be condemned, in so far as it proposes to represent modern science. For this reason, too, we think that Mr. Parker Gillmore is to be taken to task, in that he has not incorporated with M. Figuier's book the more striking results of the researches on the *Iguanodon* and *Compsognathus*. The whole book, taken *tout entier*, is not worth a single page of that most remarkable and absorbing article on the "intermedial forms between reptiles and birds," by Professor Huxley, which appeared in this journal less than two years ago. Apropos of this subject, we may mention that the editor's note on page 2, which implies a correction of a proposition laid down by Professor Huxley, arises from an absurd mental confusion of two very distinct groups of animals. On the whole, we can recommend this work as a good reading book for young people, but certainly not as a scientific treatise on its subject.

[Owing to pressure on our space, reviews of the following works are unavoidably postponed till our next number. Seeley's "Ornithosauria, Aves, and Reptiles;" Macnamara's "Treatise on Asiatic Cholera;" Bence Jones's "Life and Letters of Faraday;" and Figuier's "Earth and Sea."]

SCIENTIFIC SUMMARY.

ASTRONOMY.

A *NEBULA suspected to be nearer to us than certain Fixed Stars.*—Mr. Le Sueur has studied the celebrated nebula around η Argûs with the fine four-feet reflector by Grubb, recently sent out for the Melbourne Observatory. He finds no reason to suspect that any of the stars mapped by Sir John Herschel have changed either in situation or in brilliancy. But the nebula shows so many indications of change, that he has been led to the opinion that it lies *nearer* to us than the fixed stars seen in the same field of view. It need hardly be said that he expresses merely the view suggested to him by the appearance, and changes of appearance, of the nebula. There are no means whatever of determining whether the nebula is nearer to us or farther from us than the stars with which it seems associated. It is, however, possible, that if the nebula is really associated with those stars, the fact may be recognised by peculiarities in the apparent movements of the nebular masses. It seems difficult to study Sir John Herschel's pictures of the irregular nebulæ without being led to the opinion that stars and nebulæ are actually mixed up together. It will be interesting to learn whether Mr. Le Sueur's pictures of the great nebula cannot be explained as due to motions of the nebular masses around the stars. His present opinion is, however, that we have mentioned above.

The discovery is interesting, coming, as it does, so soon after the enunciation by Mr. Proctor of the theory that nebulæ are not external galaxies, but belong to, and fall within, the sidereal system.

Sir John Herschel, commenting on this view, remarks that possibly the nebulæ may be miniatures of our galaxy within its limits.

Method of correcting the Atmospheric Chromatic Dispersion.—The Astronomer Royal has invented a means of correcting the annoying appearances arising from the chromatic dispersion produced by the atmosphere. When observing the recent transit of Mercury, he was painfully struck with the colour and consequent indistinctness of the upper and lower limbs, both of the sun and of Mercury, suggesting the prospect of a total loss, in the observations of the transits of Venus, of that delicacy on which the success of the observations will greatly depend; "it being remarked that for the most valuable observations, the sun must be low, and the atmospheric refraction and dispersion must therefore be considerable; and, further, that the planet must be near the highest or lowest part of the sun's limb." He has found

that an efficient corrective may be obtained by applying a glass prism, of small refracting angle, in the eye-piece of the telescope. Flint prisms of 2°, 4°, 6°, 8°, 12°, and 16° effect the desired correction most satisfactorily. For use with the naked eye the prisms of lowest refracting angle must be used with the edge downwards; for view with an inverting telescope the edge of the prism must be upwards. The higher the power the larger (*cæteris paribus*) must be the refracting angle of the prism selected; so also for objects very near the horizon a prism of large refracting angle must be used.

Transits of Venus in 1874 *and* 1882.—Dr. Peters, of Altona, has re-examined the circumstances of the transit of 1874. He finds that Mr. Hind's determination of the fundamental elements of the transit are appreciably correct. He then enters into a consideration of the conditions for the successful observation of the transit. He points out that Halley's method will be better than Delisle's. In this and other respects his results agree with those deduced by Mr. Proctor. He is of opinion, further, that the observations made in 1874 will be much more trustworthy than those of 1769.

Mr. Proctor has applied to the transit of 1882 the same processes by means of which he had already calculated the circumstances of the earlier transit. The following are the deduced places where ingress and egress are most affected by parallax:—

	Lat.	Long.
(i) The place where first internal contact is most accelerated lies in .	51° 5′ S	86° 48′ E.
(ii) The place where first internal contact is most retarded lies in . .	53 16 N	94 28 W.
(iii) The place where last external contact is most accelerated lies in .	26 18 N	40 1 W.
(iv) The place where last internal contact is most retarded lies in .	23 46 S	137 11 E.

He remarks that "the corrections which have to be made in the case of this transit are less considerable than those he made in the case of the transit of 1874; not one of the above places differing by much more than 300 miles from the corresponding place" obtained by the Astronomer Royal.

The Aurora, the Zodiacal Light, and the Sun's Corona.—A series of very remarkable observations by Ångstrom, the Swedish astronomer, and by Professor Young during the recent total solar eclipse in America, seem to associate the aurora, the zodiacal light, and the sun's corona in a very singular manner. All three appear to give the same spectrum, with this difference, however, that the lines are not equally strong. Strangely enough one of the lines seen in the spectra of these lights belongs to iron. The fact that the zodiacal light and the solar corona should give bright lines at all is sufficiently remarkable, since a spectrum of bright lines is usually indicative of gaseity. However, there seems no reason to doubt the exactness of the observations, though some of the American astronomers speak of the spectrum of the corona as continuous. It is obvious that there is nothing in the fact that the coronal spectrum consists principally of bright lines, inconsistent with the fact that a portion of its light should give a continuous spectrum.

. Professor Van der Weyde remarks that the observations prove nothing as to the electric or non-electric nature of the auroral light; but prove sufficiently that that light is not derived in any way from the sun.

Triplicity of the November Meteor-stream.—Mr. Alexander Herschel considers that the observations made on the November meteors in the years 1866, 1867, and 1868, go far to prove that instead of one stream, there are three distinct streams. In each of those years there were noticed three maxima, separated by well-marked intervals during which there was a sort of lull.

We may notice, in this connection, that the November meteors by no means gave a grand display this year, in Europe at any rate. The weather was unfavourable, however, and possibly we may still hear of star-showers observed in America or Asia.

The Solar Prominences.—Dr. Zöllner has taken some remarkable views of solar prominences observed by aid of the spectroscope—with an open slit. Perhaps the most interesting of the series are six taken within the course of twenty-three minutes. The prominence observed changed figure in a most remarkable manner. Its height varied from about thirty-five to about forty seconds, or from about 14,000 to about 16,000 miles; but the changes of figure were most singular. At first the prominence was bowed towards the right, the centre of its apparent mass lying some 2,000 miles from an upright drawn through the centre of the narrow base. Ten minutes later the prominence had not only changed in figure but had become perfectly upright. It resembled at this time a tree, with an upright stem occupying about one-third of its total height. Only two minutes later the greater part of the stem had vanished and the mass of the prominence had passed over towards the left. Three minutes later a complete change had passed over the figure of the prominence; it now resembled an A, the left-hand stroke representing what had been the stem, and the down stroke abnormally thick. Eight minutes later it had again changed totally in aspect; now resembling a camel's head turned towards the right. When the enormous real volume of the prominence is considered, these changes cannot but be looked upon as highly significant. We may feel convinced that processes of enormous violence must be in action to occasion movements so rapid. Mr. Lockyer's estimates of the rate at which the solar cyclonic storms sweep on their course, are fully justified by these observed motions of displacement.

Changes of Colour in Jupiter.—Mr. Browning has noticed a series of very remarkable changes in the colour of the great equatorial belt of Jupiter. Usually, as all observers of this planet are aware, the equatorial belt is of a pearly-white colour; but recently it has exhibited a variety of colours, ranging from coppery-red to chrome-yellow, with occasional tints of green. For a considerable time its colour very closely resembled that called yellow-lake—a peculiar greenish-yellow. That these changes of colour are well-marked, is shown by the fact that they have not only been seen by Mr. Browning with his fine 12¼-inch reflector, but by Mr. Slack with a 6-inch Browning-With reflector, and by other observers with instruments of about the same size. It seems clear that processes of great interest are taking place in the atmosphere of Jupiter; and the planet ought to be carefully watched by those who possess telescopes of adequate power. One is led to

suspect that Jupiter may differ from the minor planets in respects even more important than size and rapidity of rotation. Processes of such a character as to alter the appearance of a belt which exceeds the whole surface of the earth many times in extent must be due, one would suppose, to an action affecting a large proportion of the planet's mass. It would be well worth while to enquire whether the spectroscope affords any indication of the nature of those processes which are now at work upon the leading member of the planetary system.

Eclipses of the Sun and Moon.—There will be an eclipse of the moon partially visible at Greenwich on January 17. The moon will rise shortly before the termination of the real eclipse, but her face will be clouded by the earth's penumbra for some time after. A fortnight later there will be a partial eclipse of the sun not visible at Greenwich.

Measuring Star-discs.—Mr. Proctor suggests a method of measuring the discs of those stars which are liable to be occulted by the moon, which would be worth trying by those who possess powerful telescopes. He remarks that "if the image of a star is put in rapid rotation (which may be done in many ways), while the centre of rotation moves in a circle, in a period exceeding the maximum duration of luminous impressions on the retina, the appearance presented to the eye will be that of an epicycloidal coil of light whose rapidly advancing end is always on a black ground. Now, if the eye be intently fixed on this coil just before the star's occultation by the moon, it is obvious that at the instant of occultation the whole coil will vanish; but during the brief interval which the luminous impression on the retina occupies in vanishing, there will be time to notice the appearance of that end of the coil which was last formed. Now, if we suppose the rapidity of rotation such that a quarter of a revolution was made while the moon was traversing the star's disc, it is clear that along that quadrant there will be a gradual diminution of light down to evanescence at the extremity. If the star had an apparent diameter only half as large, the arc of fading light would be but half a quadrant, and so on. The observation would be rendered easier by the fact that a neighbouring part of the coil would always give the means of a ready comparison." He finds that if the star's image rotated 100 times in a second, a star ten times as far off as α Centauri, and having a real diameter as large as our sun's, would give an arc of fading light about $55\frac{1}{4}^\circ$ in length.

New Method proposed for observing the Transits of Venus.—Professor Young suggests that the spectroscope should be made use of in observing the approaching transits of Venus. The plan he proposes depends upon the fact that the approach of the moon to the sun's limb, at the time of the total eclipse of August last, was rendered sensible before the time of actual contact, by the gradual obliteration of the bright lines belonging to the spectrum of the chromosphere. He considers that the approach of Venus to the sun's limb just before the transit of 1874 begins, and the retirement of Venus from the limb just after last external contact, might be rendered sensible in the same manner. There seems little doubt that every method by which the phenomena presented during a transit of Venus can possibly be observed with advantage, will be well studied by astronomers, and in good time. Every astronomer worthy the name will look anxiously and hopefully for-

ward to the approach of so rare and so instructive a phenomenon; and those who have the means of influencing the success of the observations or expeditions undertaken by men of science, will spare no endeavour to secure a favourable result. Cloudy weather at a few critical stations may of course render all their exertions useless. It is, in fact, impossible for astronomers, as for other mortals, to "command success;' but, judging from the past history of the science, we may feel sure that they will "deserve it."

BOTANY.

The Formation of Oil in Olives.—At the meeting of the Vienna Academy of Sciences on October 21, a memoir by Herr Harz was presented to the class on the above subject. The author thus classified his results in summarising his paper:—1. This secretion in its first formed condition does not possess the common properties of the fat-oils. 2. It is inclosed almost till its complete development in true secretion-cells, the walls of which can be made visible by re-agents. 3. These secretion-cells are not simple, but contain within themselves a number of other secretion-cells (daughter-cells), which, gradually developing, are finally converted into oil, while the membrane of the mother-cells itself also changes into oil. 4. This membrane was rendered visible by treatment with Miller's salt, or still better, first with this salt, then with aniline solution, and lastly with iodised chloride of zinc, these three reagents together colouring it a beautiful deep blue.

The Classification and Nomenclature of the Species and Varieties of Ivy.—Mr. Shirley Hibberd, who is well known to our readers as a successful horticulturist, admits three species of *Hedera.* He proposed the following subdivisions:—1. Scandent, or climbing ivies, with green leaves; 2. Scandent, or climbing ivies, with variegated leaves; 3. Arborescent, or fruiting ivies, with green leaves; and 4. Arborescent, or fruiting ivies, with variegated leaves. "Under one or more of these four subdivisions may be placed every garden ivy known." "A very brief experience with ivies will convince the observer that every scandent form tends inevitably to the fruiting form, requiring only age and immunity from the pruning-knife to produce in due time abundance of berries. It is not, however, so generally known that every fruiting form, however arborescent, is capable of reverting to its corresponding scandent form; the mere opportunity for climbing—as, for example, proximity to an old brick wall—and the roots having free range in a rich soil, tending to what may be properly termed its rejuvenescence."—*Paper before Linnæan Society,* November 18.

Chlorophyll and its Optical Properties was the subject of a communication to the Ashmolean Society of Oxford at a meeting on Nov. 29. It was, however, little more than a summary of Smith's, Stokes', and Ray Lankester's inquiries.

Spasmodic Movements in Leaves.—We learn from a contemporary that M. Lecocq records (*Belgique Horticole*) some curious movements which he has observed in the leaves of *Colocasia esculenta.* The motion is of a peculiar quivering kind, very unlike that of *Mimosa,* and is somewhat forcible.

Effects of Sunlight on Vegetation.—Experiments in reference to the effects of sunlight of different intensities in developing the chlorophyll in plants have been recently made by M. Prillieux, and have been reported by him to the French Academy of Sciences. *Galignani* gives the following abstract of the paper:—In former experiments sunlight was weakened by being received through water or glass shades, and the results were therefore liable to be modified by the absorption of rays. To avoid this, M. Prillieux, by means of a heliostat, caused a cone of light six mètres in length to fall into a dark room, and he then placed a series of five pots, each containing barley that had been reared in the dark, within the luminous region. No. 1 was placed at a décimètre's distance from the focus of the lens through which the solar rays were transmitted; No. 2 was placed at 16 times that distance; No. 3 was separated from the latter by a further interval of 12 décimètres; No. 4 was at 55 centimètres from this; and No. 5 stood at 57 décimètres from the focus. The experiment lasted from half-past one to half-past four P.M.; and at the end of these three hours it was found that all these little plants, except those of No. 1, had become green to very nearly the same extent; while those that had remained nearest the focus, and had therefore been exposed to the fullest sunlight, had remained as yellow and sickly as before.

The Affinities of the Hydroleaceæ.—According to Mr. A. W. Bennett—who has communicated a paper on the subject to the Linnæan Society (November)—the *Hydroleaceæ*, which have been severally referred to the *Convolvulaceæ* and *Hydrophyllaceæ*, must be retained as a separate order, including, however, two tribes, the *Hydroleæ* with septicidal, the *Nameæ* with loculicidal dehiscence. The tendency which exists in several species of *Hydrolea* to substitute for the normal bilocular a trilocular ovary, indicates, in the author's opinion, a closer affinity with the *Polemoniaceæ* than has been generally supposed. From *Solanaceæ* they differ by their two styles and straight embryo; from *Scrophulariaceæ*, by their regular corolla, five equal stamens, and two styles. The order is essentially tropical and sub-tropical, and especially American. The genus *Hydrolea*, which is nearly synonymous with the sub-order *Hydroleæ*, ranges from Arkansas to Monte Video, with a few Asiatic and African species, to which Mr. Bennett adds two new ones, from Tropical Africa, contained in the Kew Herbarium.

A Fungus Parasitic on the Coffee Plant.—A curious fungus, which has lately sprung up in Ceylon in the coffee grounds, has been examined by the Rev. M. J. Berkeley, F.R.S., who has sent a letter on the subject to the *Gardener's Chronicle*. It is allied to the *Mucors*, but is erected into a new genus by Mr. Berkeley.

The New Medal of the Royal Horticultural Society.—The Council of the Royal Horticultural Society has resolved on issuing a bronze medal, to be called the "Rare Plant Medal," and to be awarded at any of the society's meetings, for the first exhibition in this country of plants of great botanical interest.

Filling up Von Martius' Place at the French Academy.—At the meeting of the *Académie des Sciences*, on Nov. 22, this election took place, and several candidates présented their names. The Secret Committee, which generally practically decides these matters, gave the following as the result of its

deliberations:—In the first line, Herr Pringsheim, of Berlin; in the second line, *ex æquo*, and in alphabetical order, M. de Bary, of Halle; Mr. Bentham, of London; M. Goeppert, of Breslau; Professor Asa Gray, of Cambridge, Massachusetts, U.S.; Herr Nægeli, of Munich; and Signor Parlatore, of Florence. Out of 41 votes, 32 were for M. Pringsheim, 5 for Signor Parlatore, 3 for Herr Goeppert, and 1 for Dr. Asa Gray. M. Pringsheim was therefore declared elected.

Wave Diagrams of the Effects of Manures and other Influences on Plants.—At a very recent meeting of the Horticultural Society, Dr. Masters presented a series of diagrams showing in graphic form the relative degrees of vigour shown by the plants in the experimental grounds at Chiswick. The most striking results shown in the diagrams were as follows:—In almost every case the plants in the unmanured boxes were the least vigorous. The application of purely mineral manures was productive of little or no result in the case of the grasses, but was much more effective in promoting vigour in the case of the clovers. A striking contrast was exhibited in the case of almost all the twelve separate kinds of plants treated with ammonia salts, or with nitrate of soda respectively. It was shown by Dr. Masters' tables that almost invariably when the plants treated with ammonia salts manifested an increased degree of vigour, those treated with nitrate of soda showed a corresponding decrease. These contrasted fluctuations occurred at a time when the weather tables showed a high rainfall and a decreased temperature. Similar antagonistic results, but manifesting themselves at a later period, when the temperature was higher and the rainfall less, prevailed to a less extent in the boxes manured with a combination of mineral manures and nitrate of soda, and of mineral manures and ammonia respectively.

The Poison of the Tutu (Coriaria ruscifolia) is reputed to have been separated from the seeds by Mr. Skey. The investigations were carried on in Dr. Hector's laboratory in the New Zealand Institute

Motion of Protoplasm in Anacharis.—This is the subject of a memoir by Professor Schnetzler in the *Archives des Sciences* for September. The author tried various physical influences, and concludes that heat and light have most to do with the matter. Heat is especially active in promoting the movement; in the case of light, the most refrangible rays have the most action.—*Monthly Microscopical Journal*, November.

The Crystals of Logwood.—Hæmatoxylin, the colouring matter of logwood, has been found to be a very beautiful object for the polariscope microscope; according to Mr. Dancer, of Manchester, quite equal to salicin.

The Stems of the Arborescent Lycopodiaceæ.—In the *Monthly Microscopical Journal* for November, Mr. W. Carruthers continues his report on the above. He describes, and figures in a good plate, the structure of *Ulodendron minus.*

The Double Plate of Aulacodiscus Oreganus.—Mr. R. C. Greenleaf asserts that it can be demonstrated experimentally, that these discs are formed of two shells. He believes that many of the so-called species (even some of those described by Dr. Greville) have originated in this manner.—*Proceedings of Boston Society of Nat. Hist.* 1869.

Influence of the Hop on Fermentation.—If we are to believe the statements of Signor Tigri, the flowers of the hop arrest immediately the development of yeast.

The Mathematics of Plant-growth.—At one of the late meetings of the Botanical Society of Edinburgh (Nov. 11), Mr. W. Mitchell presented a curious paper, entitled "On Equations to the curved Outline of the Leaves of Plants." He attempts to find formulæ to express the curve of the outline, and this is how he does it:—Selecting a characteristic and well-developed leaf of any plant, he carefully traces its outline when placed on cardboard or stout paper; but when both sides appear to be equally developed, one-half the outline is sufficient. On this copy all the measurements are made. The point corresponding to the base of the mid-rib of the leaf is fixed on for the pole, or origin of measurement, and from it lines are drawn to the outline, making equal angles with each other. These lines are then measured by a scale divided on the edge into tenths of an inch; and as the first line, or radius vector, is the longest, we have a descending series of terms from which to construct a formula for the curve in question.

The Microscopic Character of Pollen Grains.—Herr Luerssen describes with considerable minuteness the pollen grains in the orders *Onagraceæ*, *Corylaceæ*, and *Cucurbitaceæ*. His memoir is to be found in the *Jahrbuch für wissen. Botanik* for 1869, and is copiously illustrated.

Eophyton explanatum.—An account of specimens of this plant found in the Lower Arenig rocks of St. David's is given (with a handsome plate) in the *Geological Magazine* for December. The following is the description of the species named above:—A raised, moderately convex stem, about four lines in breadth; widening, however, and becoming somewhat compressed at the joints. The surface is ribbed, and furrowed along its whole length. At the lower joint the ribs bend outwards, evidently to form a branch. The joint is obliquely placed, widened out, and its course distinctly marked by a deep sulcus. The cortical substance is very thin, and can be removed to show the internal structure. The internal structure is made up of compressed columns, running the whole length from joint to joint, evidently of a tubular nature, and bound together by very thin tissue.

State Recognition of Dr. Hooker's Services.—Dr. Hooker has been made C.B. since our last Number was published. The following is the notice from the *Gazette* of November 9:—"The Queen has been graciously pleased to give orders for the appointment of Joseph Dalton Hooker, Esq., M.D., director of the Royal Botanical Gardens at Kew, to be an Ordinary Member of the Civil Division of the Third Class, or Companions of the Most Honourable Order of the Bath."

The Scarlatina Fungus.—Hallier asserts that scarlet fever is the product of a fungus which he names *Tilletia scarlatinosa.* We hope it will be better established than his well-known cholera-fungus. He says he has never seen such an immense number of micrococci in the blood of any other infectious disease. These are at first as small as the finest pin-point, or the most minute granular matter. They are present in far greater numbers than the blood globules themselves; both swimming free in the serum and accumulated in granular masses and groups. They both accumulate on and penetrate into the blood globules. The white corpuscles as well as the red

globules are supplied with them, almost without exception. Just as there is a great resemblance between the seeds, roots, stems and leaves of plants, so do the microscopic germs develope themselves into sprouts and shoots which resemble those of many other microscopic plants.

Movements of the Chlorophyll Corpuscles in the Anacharis.—The following arithmetical calculation was communicated to *Science Gossip* by Mr. T. Simpson:—He examined twelve leaves from one plant with these results:—1. Leaves examined averaged in size $\frac{24}{100}$ths of an inch in length, and $\frac{7}{100}$ths of an inch in breadth. 2. Averaged 9,100 cells to each leaf. 3. 20 average cells examined in each leaf, averaged 23 chlorophyll granules to each cell. 4. Average of leaves examined shows therefore 209,300 chlorophyll granules to each leaf. At another time he examined six leaves from another plant, with slightly varying results:—1. $\frac{22}{100}$ths of an inch by $\frac{6}{100}$ths of an inch. 2. 9,750 cells to each leaf. 3. 22 granules to each cell. 4. 214,500 chlorophyll granules to each leaf.

CHEMISTRY.

Alkaline Sulphides in Bleaching.—M. Tessié du Motay, so well-known for various processes in technical chemistry, has contributed a paper on the above subject to the *Moniteur Scientifique* for Dec. 1. He supposes that the alternate action of reducing and oxidising substances accelerates the bleaching of hemp, flax, and cotton, and the tissues made thereof, and that the strength of the fibre is less impaired by this process. The substances applied as reducing materials are the sulphides of barium or calcium mixed with some sulphide of sodium, and the oxidising substance is chlorine water.

Chemical Science in England.—As a proof either of the want of energy of the Secretaries of the Chemical Society, or of the absence of workers in chemistry in this country, we may mention the very singular fact that at a recent meeting of the *Chemical Society* there was actually not a single paper to be read, and the president had to get up some discussion in order to occupy the Fellows during the time of the *séance.*

How to prevent "Bumping."—In a recent number we gave an account of a process for the above purpose. The following method, described by Schumann in the *American Journal of Pharmacy* for November, is also of interest in this direction. In distilling acids and other liquids, he proceeds in the following manner:—The end of an ordinary glass pipe, of about one-eighth of an inch opening, is shut at one end, and this end bent into a little hook; the glass pipe is then cut exactly so long as to reach from the bottom of a glass retort to within half-inch or one inch of the stopper of the tubulus. By means of the hook and a piece of twine, or a little hook of thin wire, this glass pipe is placed into the retort, the open end at the bottom, and the retort can be filled, or the retort is filled first and the glass pipe entered afterwards. If the liquid is warmed, the air in the glass tube is expanded, and constantly bubbles out at the open end; and if the boiling-point is reached, vapours of the tension of the atmosphere are formed at the spot where the glass pipe stands on the bottom of the retort, and the boiling continues regularly and quietly.

The Fermentation of Glycerine.—M. Béchamp, who alleges that chalk contains organisms still alive, and which have lived since the chalk period, states that Herr Redtenbacher has been trying the effect of these in fermenting glycerine, and Mr. Béchamp has presented an account of his experiments to the French Academy. The following mixture was made:—Pure glycerine, 250 grms.; chalk, from Sens, full of well and lively-moving microzymas; hashed mutton, quite fresh, and previously washed with cold water, 30 grms.; cold water, 3,000 c.c. This mixture was kept at a temperature of from 35° to 40° for several months. The chief results obtained by the author are, that propionic, butyric, caproic, and valeric acids are formed most abundantly: among the gaseous products, carbonic acid, hydrogen, and nitrogen are found as result of this fermentation.

Chemistry and Geology.—At the meeting of the Royal Physical Society of Edinburgh on November 24, Dr. Stevenson Macadam devoted his opening address, as president, to the above subjects, in their relation to each other. He asserted very strongly that geological phenomena—as pointed out often by Mr. David Forbes—cannot be interpreted without the aid of chemistry.

Ill Health of Dr. Anderson.—We regret to learn from the reports of the Highland and Agricultural Society that Dr. Anderson, the chemist to the Society, has been compelled through ill health to give up work for a while. It is satisfactory, however, to find that both the Society and the University of Glasgow have given him leave of absence.

Piperinic Acid and its Products of Oxidation are the subjects of a paper presented recently to the Göttingen Society of Sciences by Herr Fittig. Herr Fittig went into many details, and gave an account of the products of oxidation of piperinic acid, of piperonylic acid, of the action of nitric acid on these compounds, and of the relation of piperinic acid with bromine.

What the illuminating Power of Gas should be.—There are many of our readers who are interested in the practical chemical question, "What is good gas?" which the *Chemical News* asks in a recent issue. Giving abstract of a paper by Herr Schilling in the *Journal für Gasbeleuchtung*, the *Chemical News* states as follows:—A good and suitable gas for artificial illuminating ought to possess the following qualities:—(1) The gas ought to possess a normal illuminating power. The exact determination of the value of the normal illuminating power can only be obtained when the gas made from various qualities of coal, and manufactured according to rational principles, is compared, under exactly identical conditions, with the normal standard candle. Any gas which, by a combustion of five cubic feet per hour, exhibits a light equal to fifteen spermaceti candles, may be considered a gas of good illuminating power. (2) The gas should be absolutely free from sulphuretted hydrogen. (3) The pressure of the gas at the works and in the leading mains should, as a minimum, amount to from 0·8 to 1·0 of the water-pressure gauge.

Chloride of Gold in Crystals.—M. Debray publishes a fact of some interest as regards common chloride of gold. It is known that by heat this substance is first changed into chlorine, and the protochloride, and that then this product changes into chlorine and metallic gold. M. Debray, however, finds that if thin leaves of gold are heated in a current of chlorine to 200°

the chloride of gold, which begins to form on the gold below this temperature, is deposited in reddish long needle-shaped crystals, as voluminous as those of the volatile chlorides of molybdenum and tungsten, at some distance from the heated part.—*Comptes rendus*, Nov. 8.

Presence of Sodium Chloride in Sea-air.—It is not a novel fact that salt is present in sea-air, but the following remarks by M. G. d'Hercourt are based on some exact experiment:—From a series of observations made at Monaco, on the shores of the Mediterranean, M. Hercourt concludes that there is always on the sea-shores an atmosphere impregnated with saline particles; this layer of air has, at the above-named place, some 500 mètres' horizontal, and some 60 mètres' vertical extent. This impregnation of salt is due to what the author terms "pulverisation" of the sea-water by the breaking-up of the surf, and is not directly influenced either by barometric pressure, hygrometric state of the atmosphere, or its temperature. This hydro-mineral dust (*poussière*), as it is called by the author, is, unless there happen to exist near the coast physical obstacles in the shape of high mountains, carried far away inland, and is not to be confounded with what is of more coarse nature, and termed "spray," which is only quite local, and produced when a gale of wind blows. M. Hercourt states that, even on calm days in winter, the atmosphere near Monaco is, at least up to a height of 70 mètres and some few miles inland, impregnated with this hydro-mineral dust. There is no tide (rise or fall of water) perceptible in the sea alluded to.—See *Les Mondes*, Nov. 25, and *Chemical News*, Dec. 17.

Are there Chemical Atoms?—The Chemical Society made an effort, if not to decide the question, at least to obtain the opinion of its Fellows on the subject. At a meeting which was held on Nov. 4, and which was well attended—Sir B. Brodie in the chair—a very excellent discussion, opened by Dr. Williamson in the affirmative, took place upon the subject of the atomic theory. Those who opposed the atomic doctrine were the more numerous of the two. The "noes" seemed to carry the day. The names of the speakers were as follows: For the atomic theory, Dr. Williamson and Dr. W. Allen Miller; against it, Dr. Frankland, Dr. Odling, Dr. Mills, Sir Benjamin Brodie, and Professor Carey Foster; neutral, Dr. Tyndall.

Death of Professor Penny.—It was a very unhappy termination to the controversy respecting the claim of the late Professor Penny (Anderson's University), that of the death of this well-known labourer in the field of technical chemistry.

Estimation of the Ozone in the Atmosphere.—That the present methods of calculating the degree of ozone in the air are defective is proved by a paper lately read before the Philosophical Society of Manchester, by Mr. T. Mackereth. This gentleman made a series of yearly observations of ozone and wind-currents, and he constructed a number of tables. From them he concludes that the maximum of ozone development is coincident with the maximum of the horizontal movement of the air and of the fall of rain, and that the minimum of ozone occurred at the time of the minimum of the wind and rain-fall; and that in the summer months, when the horizontal movement of the air was at its minimum, ozone development was at its minimum too, thus proving that the existing plan of dividing ozone is open to fallacy.

The Chemistry of the process of ripening in the Grape.—At the meeting of the French Academy on Oct. 4, M. Petit sent in a memoir on the chemical phenomena of the process of ripening in the grape. The juice of the green grape contains 36 to 37 grammes of acid per litre; that of the ripe fruit 5 grammes only; and these acids have not during the process of ripening become united to bases—they have actually disappeared. The leaves of the vine contain from 13 to 16 grammes of acid per kilogramme; and, moreover, they are richer in acid in proportion as the grape is less ripe and the leaves are more green.

On Orthoxylol.—A very important paper, though one of not much interest to the general scientific reader, was lately published in the Report of the Göttingen Society of Sciences (*Scientific Opinion*, Nov. 10), by Herr Fittig. By introducing an atom of methyl into dimethylbenzol the modification of trimethylbenzol called pseudo-cumol is obtained. Pseudo-cumol oxidised by weak nitric acid yields simultaneously two isomeric monobasic acids $C_9H_{10}O_2$, which the author has named xylic and paraxylic acids. These two acids can only be different because out of the three atoms of methyl contained in pseudo-cumol, the one changed into carboxyl in the formation of xylic acid is quite different from the one changed into carboxyl when paraxylic acid is formed. This was shown to be so in this way: By decomposing xylic acid by heating it with caustic lime at a relatively low temperature, isoxylol is obtained. Paraxylic acid treated in the same way yields a new hydrocarbon orthoxylol C_8H_{10}, which differs from isoxylol and from methyltoluol. Paraxylic acid is only decomposed at an extraordinary high temperature, but the reaction goes on well, and after one rectification the distilled carbide is pure. It boils constantly at 140° to 141°, three degrees higher than isoxylol, and possesses an odour quite different from isoxylol or methyltoluol and much less agreeable. Unlike the case with isoxylol and methyltoluol it is very difficult to obtain stable nitro-compounds from orthoxylol by treatment with nitric and sulphuric acids. Dilute hydrochloric acid slowly oxidizes orthoxylol, and gives a volatile acid isomeric with toluylic acid. A mixture of chromate of potassium and sulphuric acid slowly oxidizes it.

A new Opium Alkaloid has been obtained in the course of some recent researches by Dr. Matthiessen of St. Bartholomew's Hospital. It is obtained from papaverine, and it is, we believe, a very powerful narcotic which almost rivals morphia.

The Chemistry of the Blast-furnace is the title of a good paper by Mr. Charles Schinz in the *Chemical News* of Oct. 29.

A Test for Albumen.—The following is given by M. Mehu, in the *Revue hebdomadaire*:—Take of crystallised carbolic acid, one part by weight; of commercial acetic acid, the same quantity; of alcohol, at 90 per cent., two parts; mix, and keep in a bottle. This fluid is intended to detect albumen in urine; and for that purpose, to 100 grms. of this liquid (urine) are added 2 c.c. of commercial nitric acid, and next, after thorough mixing, 10 c.c. of the carbolic acid solution. The reaction is stated to be very superior to the use of nitric acid alone.

What may be obtained from raw Spirits by Distillation.—A very useful practical paper on this subject, and one which is highly suggestive to

those who are engaged in testing the physiological properties of alcohol, was read lately before the Chemical Society of Berlin by Herren Kramer and Pinner. These chemists had an opportunity of investigating the different products which, by the distillation of spirits—that is to say, the manufacture thereof—from grain, are formed, and can be separately collected, owing to the perfection of the rectifying apparatus in use, on the large scale. Among these substances are prominent—aldehyde, acetal, propyl-alcohol, butyl-alcohol, acetic ether, fusel oil, and a mixture of different substances.

The rapid separation of Silver and Copper Nitrates.—An abstract of a paper by Dr. Palment is given as follows in the *Chemical News* for Oct. 22: The author had to prepare nitrate of silver from small silver coins which contained a large percentage of copper. The alloy is dissolved in nitric acid; the solution is filtered if necessary, and evaporated until it has the consistency of a thickish oil; when this point is reached there is added to the solution very concentrated nitric acid free from HCl. By this proceeding all the nitrate of silver is precipitated, while nitrate of copper remains in solution. One part of the concentrated metallic solution requires from three to four parts of nitric acid for the complete precipitation of the nitrate of silver; the more concentrated the nitric acid is the better, but acid of 1·250 specific gravity answers the purpose. The solution of copper is decanted off, and the nitrate of silver washed with nitric acid.

GEOLOGY AND PALÆONTOLOGY.

The Concentric Structure of Granitic Rocks.—In the Proceedings of the Boston Society of Natural History for 1869, Mr. N. S. Shaler has published a paper on this subject. Concentric lamellation differs widely from the common features of cleavage in rocks, inasmuch as, however complicated and distorted the cleavage system may be, it is always reducible to sets of planes crossing each other—if there be more than one such system—but never producing systems of curves, which are the essential feature in these fractures. This much, says Mr. Shaler, is readily seen upon the exterior of any mass characterised by this structure. Upon examining, where it has proved possible, the internal features, the interesting fact became evident that the concentric arrangement was confined to the external portions of the mass, never being discernible at a greater depth than four or five feet—rarely, indeed, below three feet—from the surface. This determination has been made from the examination of a very few sections, which were fitted for the purpose, inasmuch as, according to the author, it is by no means easy to find quarries which give sufficiently extensive sections to admit of the study of such features, which cannot be well examined in a small sectional area.

Hypsilophodon is the name given to a new genus of Dinosauria, described by Professor Huxley at the meeting of the Geological Society on November 10. The specimen on which the genus is founded was obtained by the Rev. W. Fox from the Wealden at Cowleaze Chine, in the Isle of Wight.

One of the most striking peculiarities of the skull was presented by the premaxillary bone, which seems to have been produced downwards and forwards into a short edentulous beak-like process, the outer surface of which is rugose and pitted. Professor Huxley remarked upon the known form of the symphysial portion of the lower jaw in the Dinosauria, and indicated that its peculiar emargination was probably destined to receive this beak-like process of the premaxillaries, which may have been covered either by fleshy lips or by a horny beak The dentigerous portion of the premaxilla bears five small conical teeth. The alveolar margin of the maxilla bears ten teeth, which are embedded by single fangs, and apparently lodged in distinct alveoli. The summit of the crown, when unworn, is sharp and presents no trace of the serrations characteristic of Iguanodon: but it is sinuated by the terminations of the strong ridges of enamel which traverse the outer surface of the crown. The teeth thus present some resemblance to those of Iguanodon: but Professor Huxley regarded the two forms as perfectly distinct, and named the species under consideration *Hypsilophodon Foxii*. Of the lower jaw the right ramus is present; but its distal extremity is broken off, and its teeth are concealed. On the outer surface of the lower jaw the centrum of a vertebra is preserved. Professor Huxley then stated that a certain specimen in the British Museum, which was said to be a young Iguanodon, was really the remains of an animal of this new genus.

A new Locality for Eozoon in America.—It is stated in *Scientific Opinion* that specimens of this fossil have been recently found in the serpentine of the "Devil's Den," in Essex, United States. This discovery, resulting from a visit by Prof. T. Sterry Hunt, of Montreal, to the neighbourhood, will excite new interest in the local limestone and serpentine quarry among geologists, and throw additional light upon the character and age of the rocks of that region.

M. d'Archiac's Place in the French Academy.—On November 15 the Academy elected M. des Cloizeaux in the place of M. d'Archiac in the section of geology and mineralogy. Other candidates were MM. Delesse, Herbert, Fouqué, and Hauteville. Of 48 votes M. des Cloizeaux got 40.

Fossil Crocodiles.—M. Gaston Plante has found several of these in the lower part of the Meudon clay, in the spot where the *Gastornis*, a gigantic bird, had been previously discovered.—Vide *Comptes rendus*, Nov. 15.

Earthquake and Yellow Fever.—The Persian Consul has written a letter to one of the French ministers to explain that the belief in the supposed connection between earthquakes and outbreaks of yellow fever is absolutely without foundation.

A new Form of Calamitean Strobilus has been described by Professor Williamson at the meeting of the Literary and Philosophical Society of Manchester on October 15. The details are too numerous for reproduction. The strobilus differed from those of Mr. Binney and Mr. Carruthers.

The Origin of Coal.—At one of the meetings of the Belgian Academy held during the past year, a paper was read by M. Renier Malherbe, who is connected with the mining survey of Belgium, on the various theories concerning the origin of coal. The author dwelt especially on the theory that the inflows of the sea which left marine deposits were only accidental. The paper gives tabular results of various analyses. It was reported on by M.

d'Omalius d'Halloy and M. Dewalque. The former expressed his belief that the views of the author were not in opposition to the more generally accepted opinions of geologists, i.e. that coal has been found in fresh-water marshes to which the sea has occasionally, owing to changes of level, gained access. Mr. Dewalque, in his report, is rather more severe on the author. He declines to discuss the author's opinions, or the results of his analyses, but contents himself with remarking that the author deserves to be encouraged.

The Arctic Fossil Flora.—Professor Oswald Heer, whose recent researches are familiar to geologists, read a report upon the above subject at a late meeting of the Société Helvetique. He stated that he had examined plants from North Canada, near the Mackenzie district; from Banks' Land, North Greenland, Iceland, and Spitzbergen. The fossil Arctic flora thus investigated contains 162 species. Of these, 18 are cryptogamic, 9 being very large ferns. It is noticed, too, that many of the leaves of the plants contain parasitic fungi like those of the present day. Of the phanerogamia, 31 are coniferæ, 14 are monocotyledons, and 99 are dicotyledons; 78 were trees and 50 were shrubs.

The Iron Mines of the Weald are thus described by Professor W. Boyd Dawkins, F.R.S., in a paper in the *Transactions of the International Congress of Prehistoric Archæologists* (3rd Session). The mine-pits are small circular or oval depressions, from 3 to 6 ft. wide and from 6 to 8 ft. deep. They consist of partially filled-up shafts, which varied in depth according to the thickness of the clay above the ironstone from 7 or 8 to 40 ft. They lie very close together, and are now very generally overgrown with trees; and as the ground they occupy is very much broken up, it is not yet brought under cultivation. The method of mining was to sink a shaft down to the ironstone, to remove as much ore as was within reach, then the shaft was partially filled up, and the operation repeated: and for this reason the mine-pits are so numerous and so close together that they bear a strong resemblance to the hut-circles within Celtic and Roman forts, such as those of Penknowle near Wells, Worle Hill near Weston-super-mare, Brentknowle in Somerset, and Penselwood on the Somerset border of Wilts.

The Classification of the Dinosauria.—In a paper read before the Geological Society on November 24, Professor Huxley commenced by referring to the bibliographical history of the Dinosauria which were first recognised as a distinct group by Hermann von Meyer in 1830. He then indicated the general characters of the group, which he proposed to divide into three families, viz.:—1. the MEGALOSAURIDÆ, with the genera *Teratosaurus*, *Palæosaurus*, *Megalosaurus*, *Poikilopleuron*, *Lælaps*, and probably *Euskelosaurus*; 2. The SCELIDOSAURIDÆ, with the genera *Thecodontosaurus*, *Hylæosaurus*, *Pholacanthus*, and *Acanthopholis*; and 3. The IGUANODONTIDÆ, with the genera *Cetiosaurus*, *Iguanodon*, *Hypsilophodon*, *Hadrosaurus*, and probably *Stenopelys*.

Albite in the Leinster Granite.—In some rambles during the past summer Mr. W. H. S. Westropp made the above discovery in some blocks of granite in the wall of the west pier at Kingstown, County Dublin. As the pier is built of rock from the Dalkey quarries, the mineral can be referred, with tolerable certainty, to its original locality. He was induced to look closely

at the above-mentioned blocks, through noticing that they contained a mineral of a purple colour; this proved to be fluor, which occurs sparingly throughout the Leinster granite. Associated with the fluor was a pretty considerable quantity of a white mineral, occurring in aggregations of minute crystals; this appeared to him to be so like Albite that he considered it deserving a careful examination. He brought some of it to the Rev. Professor Haughton, F.R.S., who agreed with him in thinking that it had a very albitic look, and kindly undertook to have it analysed, which has been done with the following result:—

Silica	64·70
Alumina	21·80
Potash	2·84
Soda	9·78
Loss	0·30
	99·42

Geological Magazine, Dec.

Successor to the late Mr. Jukes.—Mr. Edward Hull, M.A., F.R.S., F.G.S., has been appointed Director of the Geological Survey of Ireland, and Professor of Geology in the Royal College of Science, Dublin, in the room of the late Mr. Jukes. Mr. Hull had not long before been chosen a District-Surveyor on the Geological Survey of Scotland.

Crystals of Quartz and Crystals of Chalcedony.—In a very handsomely illustrated paper in the *Geological Magazine* for December, Mr. J. Ruskin after pointing out some of the difficulties of the particular department on which he wrote, states that one generalisation presents itself which is of great value. Whenever iron, whether oxide or sulphide, is associated with stalactitic chalcedony, it is always in the centre of the mass; but when iron, whether oxide or sulphide, is associated with quartz crystals, it is always (if determinately placed at all) either on the outside, or at a slight depth below the surface, under an external coat of clearer crystal. It may be indeterminately placed, in dispersed stars or cubes; but, if ordered at all it is ordered so. Briefly, a crystal of quartz never has a *centre* of iron, and a crystal of chalcedony never a *coat* of it.

The new French Annales de Géologie.—A new French periodical, devoted to geology and palæontology, has been commenced. It is published by Messrs. V. Masson et Fils, of Paris, and in general plan resembles the well-known natural-history journal, the *Annales des Sciences naturelles.* Its title is the *Annales des Sciences géologiques*, and the geological part is edited by M. Hebert, of the Sorbonne, and the palæontological by M. Alphonse Milne-Edwards. The first number, which has been issued this month, contains an elaborately-illustrated memoir by M. Lartet, on the Geology of Palestine. The subscription is 15 francs a year.

The new Cave at Portland.—A correspondent, writing to *Scientific Opinion* of December, gives an account of this cave. "The mouth of the cave, which is in a quarry about 50 ft. below the level of the road, is about 3 ft. wide, and is now furnished with a door. One of the quarrymen went with us and gave us each a candle. The average width is about 3 ft.; but there were a few 'rooms' from 10 to 18 ft. in diameter. The average height is about

5 ft., though in some places one could walk upright, and in others we had almost to go upon our hands and knees. The length has not been measured accurately; but our guide said it was 540 paces. It took us half an hour to get to the end, stopping now and then to look about, and twenty minutes to come straight out."

The Coal-Beds at Chanda, between Hyderabad and the Central Provinces, are said by the *Times* correspondent to be of a most inferior kind. Boring is, however, still going on, and Dr. Oldham expects a better quality. The anticipations of the survey as to quantity are not so favourable. The formation belongs to the Damuda series, which, with a thickness of several thousand feet, and more than a hundred beds of coal in three groups near Calcutta, thins out as it passes to the west, till at Chanda the coal-bearing formation has not more than 150 ft. There the coal is confined to a few beds of great irregularity near the base of the series.

The Professorship of Geology in King's College.—The election will take place at the Council meeting this month (January). Professor Rupert Jones has retired from the field, but Dr. P. M. Duncan, F.R.S., is still a candidate.

The Homologies of the Blastoidea.—A note on this subject is given by Mr. E. Billings in the last number of the *Canadian Naturalist.* It gives the following conclusions:—1. That the tubular apparatus beneath the ambulacra of *Pentremites* is the homologue of the so-called "Pectinated rhombs" of the Cystidea; that the five orifices heretofore supposed to be ovarian apertures were respiratory in their function, the larger of the five being also the mouth and vent; and that the central aperture is not the mouth, but the homologue of the ambulacral orifice of the Cystidea and Palæozoic crinoids. 2. That in the summit of the genus *Nucleocrinus* there are sixteen apertures—ten respiratory, five ambulacral, and one which is both mouth and vent. There is no aperture in the centre of the summit. 3. That *Codaster* does not belong to the Blastoidea.

Mr. Mackintosh and Mr. G. P. Scrope on Sea-beaches.—Mr. Scrope is somewhat severe on Mr. Mackintosh: in a recent paper he says:—"We all know what a sea-beach is. The Chesil bank is an admirable example. There is another stretching eastwards, along many miles, from the base of Beachy-head to Hastings. They are composed, like all sea-beaches, I believe (differing in some respects from flat sandy shores), of rolled pebbles or boulders, with or without an admixture of sand, broken shells, and other sea-wrack. Has Mr. Mackintosh examined any of his 'raised sea-beaches' and found them, or even any one of them, to be so composed? Not one! Has he produced a single sea-shell found in any of them? Not one! Throughout his whole description of these numberless banks and terraces, I cannot find that he has even attempted to examine the composition of any —with the single exception of one of a 'Series of Terraces near Llangollen.'"

A Bust of Professor J. B. Jukes.—It is proposed to have a bust in marble of the late Professor Jukes placed in the gallery of the College of Science, Dublin.

Flint weapons in considerable quantity have been found in Upper Egypt.

MECHANICAL SCIENCE.

The successful opening of the Suez Canal and of the Pacific Railway, involving an immense change in the direction of the great currents of oceanic traffic, mark an era in the achievements of mechanical science. For Londoners, the completion of another section of the Thames Embankment, of the noblest of all the iron bridges which span the Thames, and of the Holborn Viaduct, the opening of a railway through the old Thames tunnel, and the approaching completion of a new tunnel under the Thames, are results which will attest to many future generations the skill, boldness, and industry of the engineers of 1869. Now that the bridge which forms part of the Holborn Viaduct is reported safe, in spite of the unfortunate cracks on the roadway side of its beautiful granite columns, the public will perhaps forget a defect the importance of which has been overrated. It is not less to be regretted by those who are interested in scientific engineering, that so little precaution should have been taken to secure the columns of this bridge against the unequal distribution of stress due to any deviation of the direction of the thrust upon them from the vertical, or any deviation of the point of application of the thrust from the centre of the section. That there is a deviation in one or other of these respects to an extent which has caused the fissures in the columns is obvious. Mr. Muir considers that this is due to a contraction of the metal of the centre span from decrease of temperature since the erection of the bridge. This is a quite possible cause of the cracking, if it can be shown that the amount of the contraction is sufficient.

Mr. Phipps, in a letter to *Engineering*, has calculated the thrust on the columns, supposing the bridge to consist of three separate arched ribs, and finds that the nipping up of the smaller arches and the rise of the neutral lines of those arches create a couple tending to rotate the columns towards the roadway. This is perfectly correct, but Mr. Phipps has neglected the greater couple due to the excess of thrust of the middle arch which tends to rotate the columns towards the footways, and must so rotate them before the presumed rise of the neutral line could come into play. Further, the value found by Mr. Phipps for the bending action on the columns is such that the joints ought to be wide open on the footway side, which is not the case. If the columns had been loaded by three separate arches, they ought to have crushed on the footway side, whereas they have gone on the roadway side. The arched ribs are, in fact, bolted together and braced to the roadway girders. They are, therefore, more nearly in the condition of a continuous girder of three spans than of an arched bridge. Treated as a continuous girder, it is not difficult to see that the great excess of length in the middle span would tend to give the part of the girder over the columns an inclination pointing upwards over the footways, in consequence of the deflection of the middle arch; or if, from the depth of the girder at that point, an actual inclination was not produced, still the centre of pressure on the bearing would approach the roadway side.

Whatever the cause of the bending action, the report of the engineers appointed to examine the bridge seems to show that they do not consider it sufficient to cause ultimate danger to the structure.

Artificial Stone.—An interesting article will be found in *Engineering*, Oct. 22, on Coignet's "Béton aggloméré," which has come into extensive use in France, as most engineers will know. This Béton is a mixture of a large amount of sand with a small proportion of lime and cement. The proportions vary according to the quality of the sand and the rapidity required in setting. In many of the works in Paris five parts of sand, one of lime and ¼ to ½ of cement have been used. The mixture is made with the least quantity of water capable of forming a paste, in a grinding mill, and when moulded into blocks in moulds is subjected to compression by heavy blows. The result of the small quantity of water used, the thorough mixing, and compression, is a material of much greater strength, density, and uniformity of character than ordinary concrete. In Paris forty miles of sewers have been constructed of this material. At Vérinet a church, which is a complete monolith, has been similarly constructed. It has also been used for railway arches, aqueducts, tunnels, and other structures.

Communication with the Continent.—At the Society of Arts a paper has been read by Mr. Zerah Colburn, and at the Society of Engineers one by Mr. Nursey, on the important subject of communication between this country and France.

Steam Brake.—With the progressive increase of the power of the modern locomotive, and consequently of the weight of railway trains, it has become a question of the greatest importance to find means of controlling the trains after having been put in motion. It has been the practice in extreme emergencies to retard the train by reversing the engine; when that is done, the hot air passing up the chimney is drawn down the blast-pipe and pumped into the boiler against the steam pressure, and a very powerful retarding force is consequently obtained. But the reversed working is dangerous. The heated air drawn into the cylinders burns the lubricant, and the particles of soot soon cause serious cutting of the rubbing surfaces. It has occurred to a French engineer, M. Lechatelier, to arrange the engine so that the link motion may be reversed without danger to the engine, and the powerful retarding force supplied by the cylinders, when acting as pumps, may be used in the ordinary working of trains. This he effects by introducing a small jet of hot water from the boiler into the base of the blast-pipe. The water generates at once a large volume of steam, which, in the reversed action of the engine, is drawn into the cylinders in place of the heated products of combustion. Mr. Siemens, in an interesting paper on this brake, read at the Institution of Mechanical Engineers, states that this plan of working has been adopted on 1,400 locomotives on the Paris and Lyons Railway alone. It is being introduced in this country by the Fairlie Engine Company.

Velocity of Shot and Pressure of Gases in the Bore of Guns.—Captain Noble has invented and applied a remarkable apparatus by which the pressure of the gases and the velocity of shot within the bore of a gun can be measured, at any number of points during its passage. For measuring the velocity, the apparatus consists of a mechanical arrangement for obtaining a very high velocity of a recording surface, and of an electrical arrangement for registering upon the surface the exact instants at which the shot passes each point in the bore of the gun. The details of the instrument are too

complicated for explanation without drawings, but the reader may find an account of the methods adopted, in a paper by Captain Noble, in the last volume of the *Proceedings of the Institute of Mechanical Engineers.* The pressure is measured by the indentation of a block of metal by a point attached to a plunger communicating with the bore of the gun. The results obtained by Captain Noble have a most important practical bearing, namely, the determination of the quality of powder which will generate the required velocity in the shot with the least injurious action on the gun. Comparing samples of fast-burning and slow-burning powder, it was found that the time taken by the latter to project a shot a certain space in the bore of the gün was five times that required by the former. The velocity at the muzzle was nearly the same in the two cases. The maximum pressure in the gun due to the fast-burning powder was double that of the slow-burning powder.

Flow of Gases.—Professor Rankine has communicated to the *Engineer* a series of papers on the flow of gases through orifices, containing very important corrections of the formulæ hitherto accepted.

MEDICAL SCIENCES.

Eccentricity and Insanity.—As a proof of the difficulty of drawing an exact line between these two morbid conditions of the mind, the American *Quarterly Journal of Psychological Medicine* for October gives a curious case. A man made a will leaving all his money away from his relatives to found a sort of asylum for cats. The following is the last provision in the will:—" I have all my life been taught to believe that everything in and about man was intended to be useful, and that it was man's duty, as lord of animals, to protect all the lesser species, even as God protects and watches over him. For these two combined reasons—first. that my body, even after death, may continue to be made useful; and, secondly, that it may be made instrumental, as far as possible, in furnishing a substitute for the protection of the bodies of my dear friends, the cats—I do hereby devise and bequeath the intestines of my body to be made up into fiddlestrings, the proceeds to be devoted to the purchase of an accordion, which shall be played in the auditorium of the Cat Infirmary by one of the regular nurses, to be selected for that purpose exclusively—the playing to be kept up for ever and ever without cessation day or night, in order that the cats may have the privilege of always hearing and enjoying that instrument which is the nearest approach to their natural voice."

Corti's rods and their function.—Helmholtz has modified his opinion on this subject in physiology. Hitherto he regarded them as being connected with the perception of sound; but now that it has been shown that they are absent from the cochlea of birds and as many birds have high perception of sounds, the aforesaid function cannot be attributed to them.

The accessory Articular Condyles of the Skull.—At one of the meetings of the Vienna Academy during the past session, Herr A. Friedlowsky sent in a paper on the accessory articular condyles of the occipital bone of man.

An examination of 727 skulls has shown the author that these are rarely united by articulation to the first or second cervical vertebræ, and that, as a rule, they serve as attachments for the muscles or for the ligamentary connections. The author described three cases of abnormal junction of the occipital bone with the summit of the vertebral column.

Diseased Milk.—During the existence of the foot and mouth disease which prevailed so much a few months since Professor Brown examined several specimens. A long report appeared in the *Lancet.* In some specimens which were viewed with the micrometer eye-piece the milk corpuscles varied in size from $\frac{1}{2000}$ to $\frac{1}{10000}$ of an inch in diameter, and the granular masses from $\frac{1}{500}$ to $\frac{1}{1000}$ of an inch. Milk from animals affected with cattle plague and also with pleuro-pneumonia was always found to contain an abundant quantity of granular masses and pus-like bodies; and in cases of cattle plague similar elements were distinguished in the curdy exudation which existed in the mucous membrane of the mouth, pharynx, trachea, and bronchial tubes.

Buried Alive.—A paper lately appeared in the pages of *Scientific Opinion*, entitled the Physiology of Trance, by Dr. T. E. Clark, in which some very curious facts were stated. The following case of a native Indian, who was buried for a whole month, is quoted from Braid, and in these days of Welsh fasting girls may be of interest. In the floor of the house was a hole, about 3 ft. long, 2½ ft. broad, and the same in depth, or perhaps a yard deep, in which he was placed in a sitting posture, sewed up in a linen shroud, with his knees doubled up towards the chin. Two heavy slabs of stone, 5 or 6 ft. long, several inches thick, and broad enough to cover the mouth of the grave, were then placed over him so that he could not escape. The doors were closed with masonry, and a guard placed around the building. At the expiration of a month the grave was opened, and after certain processes had been gone through the Indian revived.

The Origin of Life.—A series of very valuable papers has been published recently in the *British Medical Journal.* Though the author's name is not stated, we may say that he is a most eminent *savant*, and that therefore his remarks deserve attention. He analyses the evidence on both sides of the heterogeny question. In regard to the peculiar effect of sealing vessels—a very important point in connection with this controversy—he says that we get a decided illustration of the deleterious effects of the mere sealed vessel alone, by experimenting with unboiled solutions exposed to the influence of ordinary air. The heterogenists say that, if a maceration of hay be divided into two equal parts, one of which is placed in a flask whose neck is hermetically sealed, whilst the other is placed in a glass vessel under a bell-jar of the same capacity as the flask, so that both solutions may be exposed to the same amount of atmospheric air, no ciliated infusorium ever appears in the sealed flask, whilst the fluid under the bell-jar soon contains them in abundance. M. Pouchet has also conducted a somewhat similar experiment, only one in which the possible disturbing conditions are narrowed still more. He used a filtered solution of flax in the same way, dividing it equally, placing one portion in a flask which was then sealed, and another portion under a bell-jar of the same capacity as the flask. But in this case the bell-jar was made to dip into a

stratum of mercury (which had been previously heated to 160° C); so that whilst change or renewal was prevented the two portions of fluid were exposed to exactly equal volumes of the same atmospheric air. Yet in this case also—where germs had an equal chance of gaining access to the two solutions—after eight days, monads, bacteria, and vibrios only were found in that of the sealed flask, whilst in that under the bell-jar there were also found myriads of cilated infusoria.

Brunner's Glands not follicular.—At a late meeting of the Academy of Sciences of Vienna, Herr Brücke presented a paper, by Herr A. Schlemmer, on the character and position of the intestinal glands known as Brunner's. He finds them most abundant in the horse-shoe bend of the duodenum, and he states, further, that they are not follicular glands, as has been asserted, but are truly tubular structures.

Physical Disease from Mental Strain.—A most able, graphic, and eloquent paper on this important question appears from the pen of Dr. B. W. Richardson in the *Journal of Mental Science* for Oct. We heartily commend it to the notice of all readers, both lay and professional.

Detection of Blood Stains.—The detection of blood in old and often minute stains on clothing, wood, metal, &c., can now, according to the American *Dental Cosmos*, be made with absolute certainty. The crystals of hæmatine which can be separated from the slightest traces of blood can always be recognised by means of the microscope—but to decide to what species, whether man or lower animal, the blood belongs, is a question attended with great difficulty and uncertainty. In view of this fact, Neumann has recently subjected the question to a rigid and exhaustive examination and has published his results in a book. The work contains twenty-three superbly executed coloured plates in which nineteen different kinds of blood are represented, in which the differences of the microscopic examination are displayed in a wonderfully clear manner. Neumann recommends to moisten the blood stain with distilled water and to heat to about 60° Fahrenheit on the glass side of the microscope. In this way microscopic pictures of human and animal blood are obtained of such dissimilarity that human blood can readily be distinguished from that of any other animal. The author explains in what way this is done, and gives ample illustration.

The Functions of the Ciliary Muscle.—In a recent number of the *Monthly Microscopical Journal* Dr. Lawson, after describing the anatomical relations of the ciliary muscle in a pheasant, asks:—"What effect can this muscle have on the consistence of the lens, from which it is so distant? How can it advance the lens through any action on choroid, to which its attachment is so far behind the lens? And what can be the effect of the contraction of so important a muscular structure but to bend in the border of the cornea, and thus increase the curvature of the object-glass of the eye? Its origin—the sclerotic—is not yielding; its insertion—the cornea—is. The liquid of the eye resists the inward pressure of the cornea, and driving its central part out, still more increases the curvature. Lastly, in the elastic lamina of the cornea do we not see the antagonist of this powerful muscle? Birds must necessarily possess greater power of focal accommodation than man, but why should the mechanism by which that accommodation is ob-

tained be so different from that of man as the views of Helmholtz would lead us to suppose, if we believe the ciliary muscle of birds to operate as I have suggested?"

The transmissibility of Tuberculosis.—So much has been said on the affirmative side of this question, that the views of M. Dubuisson, published in the *Gazette médicale*, are deserving attention. After describing numerous experiments, this young physiologist lays down the following conclusions: 1. The inoculated materials are generally harmless whatever may be the nature of the substances employed. 2. They occasionally produce rapidly fatal symptoms—death being apparently caused by a sort of poisoning. 3. In a few cases lobular pneumonia is observed, and should probably be regarded as the result of the inoculation; here the hepatized tissue might be mistaken for tubercles. 4. Tubercular tissue given as an aliment may occasion death, as would similar septic products. 5. Generally, however, the animals that eat of tuberculous lung experience some *malaise* from this improper alimentation, but do not become tuberculous. These conclusions, confirmed by other observers, demonstrate that tuberculosis is neither virulent nor contagious for the animals experimented upon.

The Life of Emanuel Swedenborg, by Dr. H. Maudsley, is concluded in the October number of the *Journal of Mental Science.*

A Vaccinating Machine for vaccinating babies while asleep has been invented by an ingenious American.

Have Copper-workers an immunity from Cholera?—At the meeting of the French Academy (Sept. 27), M. Burq read a curious paper on this subject. He has examined all the police statistics on this point, and he has found as a result that during the last two severe epidemics the mortality among copper-workers was only 3 in 10,000. But M. Burq has found that all copper-workers are not similarly protected, and hence he has classified artisans of this description under four distinct categories:—(1) Preservation in the first degree: opticians, dry polishers, stampers, turners, makers of musical instruments, &c. Population, 5,650; cases of cholera, 0. (2) Preservation in the second degree: founders, lampmakers, chasers, mounters, and turners in bronze. Population, 14,000: cases, 7. (3) Preservation in the third degree: gravers in copper, &c. Population, 6,000; cases, 6. (4) Preservation in the fourth degree: jewellers and clockmakers, &c. Population, 11,500; cases, 16. The workers in steel and iron, who were 28,000 in number, had 202 cases of cholera; among others the proportion was 10, 20, or 40 times higher. There is, he said, a "society for mutual assistance," which only admits turners, mounters, and gravers in bronze, including about 300 members, and during the epidemics of 1832, 1849, 1853-4, 1865, and 1866, there was only one fatal case, and that was a man who had given up his trade two years before.

Mechanical Vibration as a Mode of Cure.—Dr. G. H. Taylor has constructed a machine which is intended to send a series of vibrations through any part of the body. He has found it of great benefit in the treatment of various diseases.—Vide *New York Medical Journal*, November.

Causes of Irregularity in the Effect of Chloral.—At the meeting of the French Academy on November 2, M. Bouchut attempted to explain some of the irregularities observed in the employment of chloral in therapeutics.

He states that the chloral has not been in the same state in all the cases. As it is difficult to obtain it pure, it is often a mixed substance. M. Bouchut provides against this by preparing his own chloral from absolute alcohol, and not from the hydrated alcohol usually employed in its preparation. By this means he always obtains the same results; and he regards chloral as a powerful sedative of the nervous system, and as a narcotic likely to replace opium.

The Medical Men of Molière's days.—Molière satirised our profession so severely that a sketch of the physicians from whom he drew his conclusion is of interest. It is to be found in the *New York Medical Journal* for December.

The Physiological Action of Pyrogallic Acid.—M. Personne, in his late researches on phosphorus, was led to conclude that the poisonous properties of this substance are due to its absorbing the oxygen of the blood. He was from this led to suspect that other powerful absorbers of oxygen, such as pyrogallic acid, would produce effects on the system like those of phosphorus. His experiments—introduction of pyrogallic acid into the veins—seemed to him to prove this view conclusively. The same symptoms as those of phosphorus-poisoning presented themselves. The brown colour of oxidised pyrogallic acid was present in the tissues; and there was the usual fatty degeneration of the liver.—Vide *Comptes rendus*, Oct. 4.

The Physiological Action of Chloral.—Some curious experiments have been recorded by MM. Dieulafoy and Krishaber, which, as it were, reconcile the opinions of M. Liebreich, who says that chloral is an anæsthetic, and M. Demarquay, who alleges that it is a hyperæsthetic. These experiments seem to show that chloral—no matter what the dose—when first administered produces a period of excitation, and, if the dose is small, its action stops here. If the dose is sufficiently large, the excitation is followed by a condition of anæsthesia. Very large doses cause death, which is preceded by a marked lowering of temperature.—Ibid.

Effects of Temperature on Incubation.—In the *Comptes rendus* of Nov. 15, M. Duclos records an interesting fact in the rearing of silkworms. If the eggs are previously submitted to a very low temperature they hatch much more rapidly than when they have not been thus cooled down. Comparative experiments, so says the author, have established this fact most conclusively. The embryo of the silkworm appears therefore to need to be cooled down in order to develop advantageously, a fact which would justify the procedure practised, it is said, in China and Japan, where cold is employed in the rearing of silkworms.

How Bruit is produced.—At a late meeting of the Amsterdam Academy, M. Heynsius gave an account of researches upon the origin of certain bruits in the vascular system. In addition to the remarkable facts established by him in 1854, he has shown now:—(1) That a bruit may be produced by an increased velocity of current, even when there is no enlargement of the vessel, and in tubes, either of caoutchouc or in rigid tubes; and also (2) that it matters not whether the dilatation is below or above the contraction.

METALLURGY, MINERALOGY, AND MINING.

Lherzolite, and the Serpentine of the Pyrenees.—Mr. Townshend M. Hall, in a translation of a paper by M. E. Frossard (*Scientific Opinion*), has some interesting observations on the above subject. He seems to think that these two bodies are more closely related than is generally supposed. Speaking of the Lherzolite, he says:—The specific gravity, as determined by me from specimens obtained at Medoux, is 2·795, being slightly in excess of the specific gravity of serpentine, which ranges from 2·21 to 2·64; but less than that of augite, which varies from 3·20 to 3·38. Some of the fragments possess a laminar structure, whilst others are compact. The hardness slightly exceeds that of serpentine.

The Combination of Felspar with Soda.—In a note to the Vienna Academy Herr Tschermak gave an account of the chemical combination of felspar with soda and soda and lime. He agrees with Rammelsberg and Bunsen in regarding these combinations as isomorphous with albite and anorthite.

The Properties of Esmarkite are thus stated in a paper by M. des Cloizeaux in the *Comptes rendus*, Oct. 18. Esmarkite is a greyish-green coloured mineral; its fracture is conchoidal; hardness holds the middle between apatite and orthose; sp. gr. 2·737. Composition in 100 parts:—Silica, 47·50; alumina 33·70; lime, 15·40; magnesia, 0·56; soda, 1·84; potassa, 0·59; water, 0·94.

A New Process for removing Silica from Iron.—Mr. J. Palmer Budd describes in the *Artizan* his new process. Having pointed out the objectional nature of the existing methods, he states that his process must revolutionise the present practice in the iron trade. His usual mode of proceeding is to place a series of iron moulds similar to those used before a refinery, as near as convenient to the tap hole of the blast furnace. He makes a paste by moistening with water soft hematite ore, which, if gritty, is previously ground, and he throws a bucket-full, containing about 60 lbs., into the mould in a semi-liquid state, and spreads it evenly on the bottom and sides. The mould being quite hot from the previous cast, dries the paste, which adheres to the bottom. He then taps as much iron as is required from the blast furnace, and allows it to run over and fill the moulds to the depth of three and a half to four inches. A great ebullition takes place; jets of flame of a peculiar white colour burn on the surface, which he assumes to be combustion of silicon in the oxygen liberated from the hermatite. It was proved by repeated analysis that whilst the silicon was 1 per cent. in the white cast iron, it is reduced by this simple process to 0·200 or 0·300 per cent., or from 1 per cent. to 1-500th. A cinder is thrown up containing silicon, some phosphorus, and sulphur. The carbon is hardly at all removed. The appearance of the iron after the process is that of refined metal. For want of sufficient upward impulsive force a good deal of the scoriæ, although chemically separated in the process from the iron, is not removed from it mechanically, but is mixed with the refined metal; and, on remelting in puddling furnaces, forms a protecting slag.

The Fluids in Rock Crystals.—At a recent meeting of the Literary Philosophical Society of Manchester, Dr. Schunck, F.R.S., called attention to a

paper by Vogelsang and Geissler, on the nature of the liquids enclosed in certain minerals, such as rock crystal, topaz, and quartz, which appeared in a late number of *Poggendorff's Annalen*. The authors find that these liquids (supposed by some observers to be hydrocarbons) always consist of liquid carbonic acid, mixed, in some cases, with a little water. The experiments by which they were led to this conclusion are fully described in the paper, and, we may add, confirm the views of Mr. H. C. Sorby.

The Mineral character of Meteorites.—In a paper published in *Cosmos* for December, M. Stanislas Meunier remarks that the meteorites which arrive in these days upon the earth are not of the same mineralogical nature as those which fell in past ages. Formerly irons fell; now stones fall. In the last 118 years there have been in Europe but three falls of irons, whereas there have been annually on an average three falls of stones. The greater number of irons which exist in our collections—and they are numerous—have fallen on the earth at undetermined epochs: all the meteoric stones are of comparatively recent date. Perhaps, he says, we are even justified in saying that stones of a new kind are beginning to arrive, for falls of carbonaceous meteorites were unknown before the year 1803, and four have been observed since then. We fear these somewhat hypothetical ideas require further support.

A new Alloy.—Mr. Abel, says a contemporary, has proposed an alloy for jewellers' use, which is said to be very malleable and ductile, and to possess a fine colour. It is composed of 750 parts of gold, 166 parts of silver, and 84 parts of cadmium.

Is Nontronite a distinct Species?—This question is answered in a paper read before the Philosophical Society of Manchester on October 5, by Dr. T. E. Thorpe, who has analysed some specimens. The sample analysed was discovered, unclassified, in the Mineralogical Cabinet at Heidelberg, and was stated by Professor Blum, who was disposed to regard it as pinguite, to have been found in the neighbourhood of Heppenheim, in the Bergstrasse. Nontronite is evidently, says the author of the paper, a product of the decomposition by weathering of some silicious mineral rich in iron. It possesses a light green colour, which, on the expulsion of water, changes to a dark chestnut-brown. It is perfectly opaque, and shows no evidence of crystallisation. Its fracture is uneven, and the lustre of its streak resinous. It is unctuous to the touch, and yields easily to the nail, and is somewhat harder than talc.

What is Tiers-Argent?—The question is answered by a new American journal, the *Journal of Applied Chemistry*. It is, says this journal, the most beautiful and useful alloy of silver yet manufactured. It is made by simply melting together two parts of aluminum and one part of silver. It is hard and light, which two qualities, added to its beautiful whiteness, render it very desirable as a material for the fabrication of table furniture. Spoons, forks, goblets, castors, butter knives, and salvers, have already come into use in Paris.

The Manufacture of Iron Rails.—A very interesting paper, descriptive of the various methods now in use, was read by Mr. Williams before the Middlesborough Iron and Steel Institute, and is published in the *Artisan* for November.

Preparation of Indium in the Pure State.—In a recent number of Dingler's *Polytechnisches Journal*, a paper on this subject appears by Herren Röszler and Wolf, and is thus abstracted in the useful weekly summary of our contemporary the *Chemical News*. The authors make use of the Freiberg zinc for the purpose of obtaining indium. The zinc is dissolved in hydrochloric acid, and next boiled with excess of metallic zinc, whereby a spongy metallic precipitate is obtained of the following composition:—Lead, 1·36; tin, 0·02; cadmium, 0·13; copper, 0·004; indium, 0·15; together, 1·529 per cent. of the metallic zinc employed. This mass is first treated with sulphuric acid to remove the lead. The whitish-coloured mass is next treated with boiling water; this solution is again treated with zinc, whereby a spongy metallic mass is obtained, which is acted upon by strong nitric acid, the excess of which is evaporated, and the oxides of tin and any remaining sulphate of lead are eliminated. The residue is taken up with water, to which ammonia is added, whereby the oxide of indium is precipitated; and, after drying, this oxide is reduced to the metallic state by means of cyanide of potassium.

Non-porous Castings of Copper.—It is stated that by melting copper in plumbago crucibles previously lined inside with pipe-clay and dried, non-porous castings may be obtained. As soon as copper, while in a molten state, is in contact with coal, or carbonaceous matter, it yields a porous and spongy casting.

The quantity of Zinc obtained in 1868.—The returns obtained from the zinc mines of the United Kingdom show a production in the year 1868 of 12,782 tons of zinc ores, principally sulphide of zinc (black jack), the value being estimated at about 39,192*l.* The number of mines was 35: 18 in England, 15 in Wales, 1 in Ireland, 1 in the Isle of Man. In England and Wales the chief production was from three counties: 3,350 tons from Denbighshire, 2,858 tons from Flintshire, 2,061 tons from Cornwall. 3,278 tons were produced in the Isle of Man. The production of metallic zinc was about 3,713 tons of the value of 75,436*l.* All these figures are lower than those for the preceding year, 1867. Prices were lower in 1868 than in 1867 in the London market, the mean price of spelter falling from 21*l.* 6*s.* in 1867 to 20*l.* 6*s.* 4*d.* in 1868, and the mean price of zinc (in sheets) from 27*l.* 7*s.* 6*d.* to 25*l.* 13*s.* 4*d.* per ton.

Augite and Amphibole.—A paper relating to these will be found in the proceedings of the Chemical Society of Berlin, No. 13.

Durangite is the name of a mineral from Durango in Mexico, and described in Silliman's *American Journal* for September.

MICROSCOPY.

For progress in general histology we must refer our readers to the sections of Geology, Botany, and Zoology. In reference to microscopic progress, we can do no better than state that in the three past numbers—October, November, and December—of the *Monthly Microscopical Journal*, the following highly important contributions will be found:—

On the Structure of the Stems of the Arborescent Lycopodiaceæ of the Coal-Measures. By W. Carruthers, F.L.S. *Illustrated.*—On the Development of the Ovum of the Pike. By E. B. Truman. *Illustrated.* —On the Structure of the Stems of the Arborescent Lycopodiaceæ of the Coal-Measures. Part II. *Illustrated.* By W. Carruthers, F.L.S., F.G.S., Botanical Department, British Museum.—Experiments on Spontaneous Generation. By Edward Parfit, Curator of the Devon and Exeter Institution. *Illustrated.*—Further Remarks on the Nineteen-Band Test-Plate of Nobert, and on Immersion Lenses. By Colonel Woodward.—On High-Power Definition, with Illustrative Examples. By G. W. Royston-Pigott, M.A., M.D.—On the Presence of Foraminifera in Mineral Veins. By Charles Moore.—On the Relations of the Ciliary Muscle to the Eye of Birds. By Henry Lawson, M.D. *Illustrated.*—The Histology of the Eye. By John Whitaker Hulke, F.R.S., F.R.C.S., Assistant-Surgeon to the Middlesex Hospital, and Surgeon to the Royal London Ophthalmic Hospital. *Illustrated.*—On Collecting and Mounting Entomostraca. By J. G. Tatem.—My Experience in the Use of Microscopes. By Dr. H. Hagen.—Further Remarks on the Plumules of Battledore Scales of some of the Lepidoptera. With 3 plates. By John Watson.—The Development of Organisms in Organic Infusions. By C. T. Staniland Wake, F.A.L.S.

We would especially call attention to Dr. Pigott's paper, because, if its views are correct, it opens up quite a new period in microscopy, in alleging that even our best objectives are imperfectly constructed for spherical observation.

PHOTOGRAPHY.

A new Artificial Light for Photographic Enlargements.—The production of large photographs of a size suitable for suspension on the wall, either in their uncoloured state or after they have been worked upon in crayons or oil colours, has for a considerable time received much consideration from photographers. When an attempt is made to produce a very large portrait direct from the sitter, so numerous are the difficulties that supervene, that very rarely indeed is the effort crowned with success. All the difficulties of focus, projection, and manipulation are happily got rid of by taking a small negative, *carte de visite* size, and placing it into a solar camera or a magic lantern to be "enlarged," which it may be in a very satisfactory manner when the light is powerful and the enlarged image is thrown upon a sensitive sheet of paper instead of the usual screen.

As the solar light cannot be relied upon, it becomes a matter of importance to secure the best possible artificial light. The lime light is very steady and its brilliancy is undoubted, but unfortunately its actinic value is low. Magnesium, on the other hand, does not yield a steady light, but its actinic power is very great. Within the past two months Dr. Van Monckhoven has been demonstrating before some of the photographic societies, and among others that of London, the possibility of uniting in one light the good qualities of both the lime and the magnesium lights. This union he effects

by substituting for the lime, in the former of these sources of illumination, a piece of carbonate of magnesia, or, more strictly speaking, a mixture of carbonate and chloride of magnesia. The mechanical and even the gaseous portion of the lime light remain as they are; the only modification required to render the oxyhydrogen light thoroughly useful for photographers being the substitution of the magnesia as above described.

In less than three minutes Dr. Monckhoven produced an enlargement on a sheet of paper from a carte negative; and with an exposure of only six seconds he was enabled to impress a wet collodion plate so as to yield a good picture. Although this light is not more powerful than that produced by the combustion of magnesium ribband or wire, it is so actinic when compared with the lime light that its introduction will not fail to give an immense impetus to the production of life-size photographs by the magic lantern, or as photographers now designate it, the enlarging apparatus.

Partial Intensification of Negatives.—A process by which negatives slightly over exposed and deficient in general intensity may be so improved as to yield good proofs has been published by Herr Meissner. The treatment is one that in the hands of an artist of taste is susceptible of yielding some excellent effects. After fixing with cyanide of potassium the negative is varnished. Those parts, such as the whites, that do not require strengthening are carefully painted over with a solution of gum arabic, to which any convenient colouring matter is added so as to permit every touch to be seen. Thus prepared the plate is placed in a vessel of methylated spirits of wine, by which the varnish is removed from those portions not protected by the gum arabic; and after the plate is washed it is intensified in the usual way, the whites being protected from the action of the intensifier by the coating of varnish. In this way may be obtained very artistic effects. After the intensification has been completed the plate must be varnished.

How the Eclipse of the Sun was Photographed.—Some very superior negatives of the last solar eclipse were obtained by a party of American photographers, who operated with the large equatorial telescope of the United States Naval Academy, Annapolis. This instrument has an object-glass seven and three quarter inches clear aperture, and nine and a half feet focal length. It was fitted with a camera capable of taking plates seven inches square. On these were thrown (the eye-piece of the telescope being also used) images of the sun four inches in diameter. The camera was fitted with an instantaneous shutter capable of being acted upon by a hair trigger. One hundred and twenty-two negatives were obtained. Seven negative baths were employed. The duty of one operator was to coat the plates with collodion and immerse them in the bath; another took each plate out, wiped its back, put it into the plate holders (of which there were several) and passed it out to a third operator, who placed it in the camera, adjusted the telescope and exposed, noting the time by a chronometer placed at his elbow, and then returning it to a fourth operator inside, whose duty was to develop the image and number the plates with a diamond. By this even distribution of labour, and by the help of every necessary kind of mechanical appliance, negatives were taken at the rate of one every minute of the eclipse, or a hundred and twenty-two altogether. The chemicals used were of the usual kind; the collodion contained cadmium and ammonium; the negative bath

was of the strength of forty grains to the ounce, and was acidified with nitric acid. The iron-developing solution was weak; and the plates were fixed in hyposulphite of soda.

New Alkaline Dry Process.—A new dry process, which has been worked out by Mr. F. Maxwell Lyte, and has lately published, possesses the following advantages over many of those hitherto used:—The negatives are characterised by great fineness, with delicacy of detail, and a total absence of stains; the plates are prepared with great rapidity and in an ordinary negative silver bath; and, lastly, they keep well even in the most sultry weather.

To prepare the plates, coat with collodion and sensitise them in the usual bath, and wash first in water, next in common salt and water, and, lastly, in plain water again. Now coat them with the preservative solution which is composed of

Albumen . . .	2 parts
Water	2 „
Spirits of wine . .	1 part.

The spirit must be mixed with the water previous to its being added to the albumen. After being whipped together and strained, when about to be used a sufficient quantity of this is mixed with a sixteenth portion of its bulk of ammonia saturated with chloride of silver.

When the plates have been coated with this preservative they are thoroughly dried and stored away till required for use. The development is effected by a plain one per cent. solution of protosulphate of iron, by which the image will be rendered very faintly visible; but any degree of intensity may be obtained by a subsequent application of pyrogallic acid and silver.

Removal of Silver Stains.—Many methods for the removal of stains from the fingers of the photographer have been published, but the latest discovery in connection with this subject will probably prove to be not the least valuable, seeing that no cyanide of potassium is required:—Immerse the fingers in a saturated solution of sulphate of zinc slightly acidulated, and then rub the stains with a bar of metallic zinc, in order to facilitate the reduction of the silver or iron of which the stains are composed. The salts of zinc and iron which have penetrated the skin are decomposed by the zinc salt, and all the zinc salts are white and soluble. When the last trace of black has disappeared, the hands are first rinsed in plain water and then washed in soap and water.

A Portable Camera for Dry Plates.—Photographic tourists are now likely to get a camera in which are combined all their requirements. A patented instrument has been exhibited, and is now being manufactured, capable of containing a camera and lens, twenty-one sensitive plates ready for exposure, and all other requisites, the whole size being eight inches high by five inches deep. The chief peculiarity of its construction is that the lower portion of the cabinet contains the sensitive plates, which, by the action of two handles, are each brought up to be exposed in the camera, and afterwards stored away, permitting each plate of the whole series to be thus treated with a degree of certainty from which no hitch can be anticipated. It has secured the admiration of all who have seen it, and is likely to come into general use during the next season. Mr. Walter Cook is the inventor of this ingenious camera.

PHYSICS.

The Superficial Tension of Liquids considered in connection with certain movements observed on their surface, is the title of a memoir presented to the Royal Academy of Belgium by M. Van der Mensbrugghe, and published in the *Bulletins* for 1869. The paper is of particular interest for those who are acquainted with Mr. Tomlinson's researches in this country. The movements described and explained in this memoir are such as those seen when a particle of camphor is placed on a surface of water. Such were explained by M. Dutrochet, on the supposition of a peculiar force, which he could not explain, and to which he gave the name of *epipolic force.* The author shows that this force is nothing more nor less than tension, and he proves his case by numerous experiments. Thomson had shown, as early as 1855, that the cause of the repulsion seen when a drop of alcohol was placed on water was the excess of tension of the latter; but he confined himself to this one phenomenon. Dupré of Rennes, in 1867, showed that when one portion of a liquid is heated, it is drawn in all directions, simply by means of the difference of tension thus produced. The author has extended these observations very considerably, and has thus shown that a great variety of phenomena, hitherto unassociated, are really very closely allied.

The Reversion Spectroscope.—A description of this curious instrument, by Herr Zöllner appeared in *Poggendorff's Annalen*, Nov. 9, 1869. For the benefit of those who cannot read German comfortably, we may mention that a translation into French appears in the December number of the *Archives des Sciences* of Geneva.

The British Meteorological Office.—We cannot hope for much advance in Physical Meteorology till better communication with other countries is established. Meantime, however, much is being done; though there is yet a want of uniformity. In the report of the Meteorological Committee to the Royal Society it is stated that the system of telegraphy of storms to foreign countries is not uniform in its character. To the Marine Ministry in Paris the same messages are sent as to our own south coast and to Jersey. Holland and Hamburg receive special telegrams similar to those which are sent to the Underwriters' Association of Liverpool. They convey the amount of atmospherical disturbance whenever the difference of readings over the same definite area exceeds 0·8 inch. The authorities at Hamburg hoist a drum at that port and at Cuxhaven whenever a telegram is received from London. These stations are thus precisely similar to those on our own coasts. In return for this intelligence immediate intimation is sent to London from France, Holland and Hamburg, by the meteorological authorities in their respective countries whenever a storm strikes any part of their coasts. The arrangements for telegraphy of storms to Italy have been interrupted by the lamented death of Professor Matteucci in the course of the year.

The Spectrum of the Aurora.—It is interesting to note that, in confirmation of Angström's researches, Mr. D. K. Winder, of the Toronto Astronomical Society, has found results closely allied to those of Angström's. The report of the meeting of this society in October, which appears (with a

woodcut of the spectrum) in *Scientific Opinion*, states that Mr. Winder had submitted the aurora to careful spectrum analysis, and by a peculiar arrangement of the slit of his spectroscope had succeeded in obtaining a beautiful spectrum, consisting of one very bright line in the yellow, and one of less brilliancy in the green. The bright line was close to the sodium-line D, and coincident with an air-line which appears in the solar spectrum when the sun is near the horizon. The pale line in the green he failed to identify as belonging to any known substance.

Influence of Different Coloured Lights on Mineral Oils.—M. Grotowsky publishes some curious results in the *Moniteur de la Photographie*. The conclusions are as follows:—1. The photogenic or solar oil preserves all its properties intact when inclosed in iron barrels. 2. When inclosed in vessels of white glass and protected by straw, little and but slightly injurious modification was apparent. 3. Vessels of white glass painted black were not sufficient to protect the oil from absorbing traces of ozone, but the stoppers were not attacked. 4. Vessels of white glass, unprotected.—In these the oil became much charged with ozone, and burnt very badly: in colour it became of a very marked yellowish tint, and its density increased by 0·003. 5. Vessels of green glass, unprotected.—The oil became charged with ozone, but burnt well notwithstanding; the cork stoppers were attacked, and the colour of the oil became modified. 6. Vessels of green glass, painted black. —Ozone was absorbed, but no apparent modification was appreciable. 7. Vessels of green glass covered with straw.—Traces of ozone were apparent, and the oil assumed a slight yellowish tint. No defect was, however, to be observed in burning. 8. American petroleum inclosed in vessels of white glass, unprotected, was much charged with ozone, and would scarcely burn in consequence; it became of a yellowish tint, and its specific gravity was augmented by 0·005. 9. The same oil screened from the access of light underwent no change.

An International Meter.—At the meeting of the Académie des Sciences of Paris on Oct. 25, M. Dumas made some observations on a letter received from M. Jacobi, in which the Russian *savant* announced that there had been received an official reply from the Academy of Berlin to that of St. Petersburg. The Berlin Academy is in agreement with that of St. Petersburg and that of Paris in regard to the necessity of employing the *mètre* in the Archives of the latter; but it suggests that there should be prepared, under the inspection of an international commission, a number of exact counterparts which would serve as standards for the future.

The Ratio-Micro-Polariscope is the name given by Mr. J. J. Field to an ingenious contrivance for exhibiting the effects of polarised light under the microscope. Its description is too detailed for insertion here, however. The object of the inventor is to obtain a plan by which a definite effect with selenites may be always had. This he achieves by a combination of toothed wheels numbered so that the actual revolutions made in obtaining any separate tint are recorded, and thus in future observations the difficulty of experimenting with any particular colour is obviated.—*Journal of Quekett Club*, Oct.

Absorption of Hydrogenium by Iron precipitated by Electricity.—The *Chemical News* (Dec. 17) in one of its recent summaries gives an abstract of a paper on the above subject by M. Jacobi of St. Petersburg. Iron obtained

galvanically is stated to be a silver-grey coloured, velvety-looking, very finely-grained mass, of 7·675 sp. gr. at 15°; this metal is so hard that it scratches glass, but is, also, very brittle. When this metal was very carefully ignited in a covered platinum crucible, its colour became deeper, its hardness and brittleness were lost, and its sp. gr. had increased to 7·811, which is higher than that of wrought iron. The fact of this increase of specific gravity led the author to conclude that the iron might contain gas absorbed in its substance. A quantity of 9·730 grms. were treated in a Sprengel's aspirator, with the result that, aided by an increased temperature, which was carried to dull-red heat, 17·76 volumes of gas, chiefly composed of hydrogen, were obtained from the quantity of iron submitted to experiment. Vide *Bulletin de l'Académie impériale des Sciences de St.-Pétersbourg.*

The Value of Mercurial Thermometers.—M. Bosscha lately sent to the French Academy an account of a mercurial thermometer of a peculiar form. M. Regnault in commenting on it thinks it is impossible to lay down rules for the registration of mercurial thermometers; the only exact instrument suited for experiments requiring precision is the air thermometer. This is, however, an inconvenient instrument, and therefore M. Regnault recommends that it be used only as a standard with which to compare the mercurial instruments.—*Comptes rendus*, Oct. 18.

The Electric Resistance of Platinum.—Herr Stefan, in a note to the Vienna Academy on the electric resistance of plates of platinum, stated that the results of his experiments accord with the theories of Kirchhoff.

The Office of the late Professor Graham.—It is unfortunate for science that another of its few rewards has gone. The Mastership of the Mint is an extinct office. No successor to Graham will be appointed.

Graham's Labours will form the subject of a Friday evening's discussion (before Easter) by Dr. Odling, at the Royal Institution.

Lectures on Light.—Dr. Tyndall's "juvenile" lectures commence just as we go to press, and will be continued for some days during the month of January.

The Rotation of a Rigid Body about a Fixed Point is the title of the paper by Professor P. G. Tait, which has just obtained the Keith medal of the Royal Society of Edinburgh.

Examination of Transparent Substances by Polarised Light.—A very instructive paper, from which our microscopical readers may gather a hint or two, was lately laid before the French Academy by M. Lallemand. The author gives instructions as to how to obtain several polariscopic effects. In order to see the illumination of any fluid—water, for instance—it is poured into a glass tube, which is closed at both ends by means of pieces of plate glass: the tube is then placed in a dark chamber in a horizontal position, and a ray of polarised light is made to shine upon the tube, which, when looked at sideways, will exhibit a vivid glimmering; but, seen in any other direction, the tube remains dark. Hence, it follows that the propagation of the light takes place in the direction of the plane of polarisation; and it also follows that the vibrations of the waves of the ether take place in the perpendicular direction. When, instead of water, any other fluid is taken which is endowed with rotatory power—a solution of sugar, for instance—a very beautiful chro-

matic effect is displayed; the colours of the spectrum will be seen distributed in an elliptical shape. Cylinders of solid glass, submitted to the same experiment, exhibit somewhat similar phenomena, but less conspicuously than fluids.—*Comptes rendus*, Oct. 25.

Electric Phenomena of the Solution of Salts in Water.—M. Raoult in a paper in the *Comptes rendus* says that the dissolution of a salt in water is a complex phenomenon whereby we distinguish—1st, the *fusion* (melting), or *disintegration* of the salt, whereby heat is absorbed; 2nd, the *diffusion* of saline molecules in water, which also absorbs heat; 3rd, the *combination* of the salt with water, whereby heat is set free. The author then states that the conditions alluded to under Nos. 1 and 2 do not produce any electricity, but that, on the other hand, the combination of a salt with water certainly does give rise to an electric current, to prove which the author records a series of experimental results.—*Comptes rendus*, Oct. 11, and *Chemical News.*

The Force of Sea-waves utilised.—At the *Société philomathique* of Paris, on October 23, a paper was read by M. de Caligny on a new application of centrifugal force in one of his machines for effecting drainage by means of sea-waves. In this apparatus, already described to the Society, the water enters from the marsh to be drained into the apparatus at each oscillation occasioned in it by the waves, and cannot return in consequence of the closing of a valve. The present paper describes the various arrangements of the apparatus to suit different circumstances. Drawings of this apparatus are to be seen in the recent numbers of M. de Cuyper's *Revue universelle.*

Peculiar Luminous Effects.—M. E. Becquerel has published a fifth memoir of his researches on the luminous effects produced by the action of light upon bodies. This memoir treats of the refrangibility of the active rays. He has endeavoured to study the action of rays of different refrangibility on phosphorescent bodies. He has now established, with different substances—alumina, lime-salts, uranium-salts, &c., as he had already done with the sulphides of the alkaline earths in which the phosphorescence persists for a long time—that each substance behaves in a particular manner, and presents one or more active spaces unequally distributed in the luminous spectrum; and, further, that it is the base of the impressionable chemical compound which appears to give its specific character not only to the emitted rays, but also to their refrangibility, and to the extent of the active space.—*Comptes rendus*, Nov. 15.

Magnetic Storms and Auroræ.—At a late meeting of the Academy of Sciences of St. Petersburg, M. Wild reported upon the auroræ boreales of April 15 and 16, and May 13 and 14 last, and on the relations which exist between these phenomena and magnetic perturbations. On April 14, a very severe magnetic storm was recognised by the people in charge of the electric telegraphs, but the cloudy condition of the sky prevented the auroræ being seen, and thus the cause recognised. All the directors of the Russian observatories reported simultaneous magnetic disturbances of a similar order. M. Wild remarked also that similar perturbations were recorded in France, England, Italy, and all over Europe. The aurora of May 13 furnished another example of the same kind. On this occasion M. Rudneff called M. Wild to show him the extraordinary oscillations of the magnetic needles. In spite of an attentive observation of the northern sky, no trace of aurora

was seen, probably owing to the cloudy state of the sky. But the author remarked there is now no doubt that on that evening a very splendid aurora was seen at Moscow and other observatories better placed for the purpose. M. Wild then gave long quotations from the reports of MM. Struve, Wagner, Gylden, and others.

How to prove the Constitution of Flame.—In the *Chemical News* of October 8, M. Dufour describes an ingenious method of doing this with an ordinary gas jet.

Does Light alter the Colour of Glass?—M. Bontemps says, Yes. M. Bontemps has found that all glasses with a soda base are coloured in a very short time by sunlight. It is different in the lead glasses. The ordinary green glass loses this green colour in a few years, and becomes yellow, and then red or violet. Glass with an azure hue and that with a lead base do not change colour under like circumstances.—*Comptes rendus*, Nov. 22.

ZOOLOGY AND COMPARATIVE ANATOMY.

Dr. Carpenter's Expedition.—At one of the recent meetings of the Royal Society, Dr. Carpenter gave a sketch of his report on the results of the deep-sea expedition. Perhaps the actual dredgings constitute the most reliable portions of the novelties of this report, and these belong mostly to this section of our summary. They include silicious sponges and foraminifera, together with zoophytes, echinoderms, molluscs, annelids, and crustaceans. One hundred and twenty-seven species of mollusca not previously known to exist in British seas were among the captives, and a large number of these are altogether new to science. The expedition has nearly doubled the number of British echinoderms, and at one spot, where the dredge brought up little or nothing, and where Captain Calver devised a plan for sweeping the bottom with hempen tangles, the first haul of these tangles secured, at a moderate estimate, 20,000 specimens of a single form of echinus. In the cold area *arenaceous foraminifera*, creatures which construct habitations by the agglutination of particles of sand, were so abundant that it will be difficult to find names for the new varieties. Many new sponges, some differing widely from previously known varieties, were also discovered.

Fresh-water Crustacea.—A very valuable memoir has been presented to the Belgian Academy by M. F. Plateau, and has been specially reported on by the elder Van Beneden. The subject of the memoir is the Natural History of the Fresh-water Crustacea of Belgium, a point to which M. van Beneden and others have lately given much attention. In Part I. of his paper M. Plateau dealt with the genera *Gammarus*, *Lynceus*, and *Cypris*. In the second part he has investigated the genera *Daphnia*, *Bosmina*, and *Polyphemus*; and in the third he treats upon *Cyclopsina*, *Canthocamptus*, and *Cyclops*. The author shows that in the *Daphniæ* there exist beneath the test and valves all the parts which enter into the constitution of the carapace of Decapods, and he considers the two morphologically identical. The valves he considers as an appendage of the fourth somite. The reporter, however, takes exception to these opinions,

alleging that as M. Plateau has not founded his homologies on development, they have a very questionable justification. The author has also described the phenomena of the moult, and the relations of the venous sinuses round the heart. He shows, too, that the form of the heart is not always that which M. Leydig described in *Daphnia macropus.* M. Plateau, in the third part of his memoir, goes into the morphology of Cyclops. He here recognises four thoracic, six abdominal, and six cephalic somites. Hence he differs from Huxley, Spence Bate, and other carcinologists in admitting only sixteen instead of twenty-one somites. The author then describes the muscular system. The other details are of equal interest, but we have not space to reproduce them.—Vide *Bulletin de l'Acad. roy. de Belgique*, 1869.

The Formation of Coral Reefs.—In the discussion which followed Dr. P. M. Duncan's paper on Physical Geography, elucidated by coral-faunas (at the Geological Society, Nov. 24), Professor Alexander Agassiz accounted for the circumscribed area of many corals in the Atlantic from the young of many coral species attaching themselves within a few hours of their becoming pelagic. He traced to the great equatorial current, which must have traversed the Isthmus of Panama and the Sahara in a pre-cretaceous period, the distribution of certain forms, which the rising of the Isthmus of Panama eventually checked. He thought that the limits of the depth at which true reef-building corals existed would be considerably extended in consequence of recent discoveries by means of dredging. He mentioned the formation of a reef at the present time off the coast of Florida, which threw light on the manner in which mudflats were formed, and the sea eventually filled.

Relation of the Chinese and Polynesians.—At a meeting of the Ethnological Society on Nov. 9, Professor Huxley, in the course of a discussion, referred to the similarity between certain Chinese customs and those of the Polynesians; such as the exclusion of a word occurring in the name of a great chief. In like manner the prohibition of marriage between persons of the same surname is a custom common to the Chinese and the Australian. He also referred to the popular but erroneous notion that the Chinese are modified Mongols, and pointed to the fact that, although both had long black hair on the head and only scanty hair on the face, yet the Chinese had a long skull with prominent brow-ridges, whilst the Central Asiatic had a broad skull deficient in brow-ridges.

The Phyllopoda in America.—At the last meeting of the American Association for the Advancement of Science, Professor Verrill, who gave an account of the natural history of *Branchipus* and *Artemia*, stated that he has obtained two new species, the latter one from Mono Lake, California, and the other in numerous individuals from tubs of salt water on a railroad bridge near New Haven.

The Theory of Natural Selection.—Mr. Darwin's views receive a severe but clever critique in a series of papers published in the *Month*, a literary journal, in the months of June, July, and August last. The papers, we have since ascertained, are by a most eminent naturalist, who, however, has been too modest to attach his name to them.

Pangenesis has found a formidable opponent in Professor Delpino, an

Italian *savant*. Delpino's papers are translated in full in *Scientific Opinion* for November, and this journal also contains Mr. Darwin's reply. It would be impossible for us to abstract either, but those who are at all interested in philosophical zoology should read these papers for themselves.

Pseudo-Scorpions.—One of the best, thoroughly zoological and anatomical, papers on this group which we have met with for some time is that of Mr. McIntyre, in *Science Gossip* for November. It is full of illustrations, and gives the practical observations of one of our earnest workers in the natural history of this and kindred groups. Physiologically the facts on fecundation recorded are of importance.

The late Professor Sars.—Since our last number Science has lost one of her most distinguished and devoted students. The Professor of Natural History in Christiana has passed from among us. His labours are too well known to need any comment of ours.

The Anatomy of the Alcyonidæ is the subject of a memoir presented to the French Academy on Nov. 22, by M. Pouchet.

New Species of Fishes.—Herr Steindachner, continuing his researches, has presented to the Vienna Academy a description of the following nine new species of fishes, forming part of the Imperial Museum of Vienna: *Genyoroge canina, Mesoprion guttatus, Heros Jenysii, Clupea* (*Alosa*) *notacanthoides, Clupea setosa, Leptocephalus maculatus, Leptocephalus Peruanus, Solea Mazatlana, Abramocephalus microlepis.* The characters of the new genus *Abramocephalus* are: form of body like that of species of the genus *Hypophthalmichthys*; pharyngeal teeth in a single row, four on each side, on the vertical ramus, *cuillère*-shaped, masticatory surface finely striated; belly sharply carinated.

The Law of Symmetry in Animals.—A lecture on this interesting subject was delivered some time ago before the Royal Dalton Society, by Professor Macalister, and was reproduced in *Scientific Opinion* (Nov. 17). Dr. Macalister enters into some questionable mathematical calculations.

The Polypterus of the Senegal.—M. Dumeril called the attention of the French Academy (Oct. 18) to a work in German upon the fishes of the genus Polypterus of Senegal, by Herr Steindachner of Vienna. One of the species described by the German naturalist resembles the Polypterus of the Nile described by Geoffroy Saint-Hilaire, but differs in certain peculiarities. Its length was from eighteen to nineteen Vienna inches, it possessed a transition respiratory organ, and a gill with a single branchial tuft. In one other species—that of Cuvier—the branchiæ also exist, but they disappear when the fish reaches a length of four or five inches. These fishes, according to M. Dumeril, approach the *Amphibia Urodela* known as Axolotls.

The Development of the Ovum in Mysis.—The very valuable memoir on this subject by M. Ed. van Beneden is published in the *Bulletins* of the Belgian Academy. The following are the leading conclusions deducible from the author's elaborate enquiries:—1. The blastoderm is formed in the course of a partial segmentation of the vitellus. 2. The cellular zone which results from the multiplication by division of the cicatricule extends itself over the whole surface of the egg, to form a closed blastodermic vesicle, before any trace of organs is seen. 3. The division of the embryo into a

cephalic and a caudal lobe results from the division into two laminæ of a primordial cellular fold, which may be compared to the cellular column (Keimhügel) of *Hemiptera*, *Orthoptera*, and *Lepidoptera*. The identity of formation of these organs, which have evidently the same morphologic value, and perfect analogy between the phenomena which subsequently take place in the cephalic lobe in the crustacea on the one hand and the insects on the other, are in my opinion facts which enable us to determine with ease and certainty what are the homologous organs in these two groups of *Arthopoda*. 4. The caudal appendage of Mysis is folded under the abdomen as in all Decapods. 5. The caudal lobe commences to be formed before it is possible to recognise the least trace of antennary appendages: these appear at the same time as the mandibles in the form of simple cellular papillæ. 6. The Nauplian cuticle is formed at once over all the surface of the embryo: it is the first embryonic cuticle. Mysis does not undergo a blastodermic moult. 7. The tail, which is bifid in some species (*M. vulgaris* and *M. chameleo*), is simple, and terminates in a cul-de-sac in other species (*M. ferruginea*).

The Anatomy of the Aaard Wolf (Proteles cristatus).—Among the many excellent papers read during the quarter before the Zoological Society was one by Professor Flower on this subject. The result arrived at, after a careful examination of every part of this animal, was that *Proteles* constituted of itself a distinct family of carnivorous animals, allied to the *Hyænidæ* and *Viverridæ*, but more closely to the former than Mr. Flower had previously supposed when he had only the skull of this remarkable animal to judge by. Mr. Flower's paper was illustrated by the exhibition of the stuffed skin, a complete skeleton, and a full series of anatomical preparations of the internal organs of this animal, all taken from the same individual.

Death of Herr Kner.—Herr Kner, of the University of Vienna, has died since our last.

The Brighton Aquarium is likely to be soon *un fait accompli.*

Improvements in the Dredge.—Captain Brabazon has taken out a patent for the improvement of the oyster dredge which deserves the notice of naturalists. The links of the new dredge are runners like those of a snow sledge. They are turned up at the ends, and connected by an iron ring or link, which rests on them and presents a smooth under surface, which carries it along the bottom of the sea, passing over the spat and brood of oysters, mussels, &c., without injury to them.

The Variations of the Human Skull.—An important paper was read by Professor Cleland before the Royal Society at one of its meetings ere the close of the last session. The author's conclusions alone (in abstract) would fill a couple of our pages. We must, therefore, content ourselves with commending the paper to the attention of anatomists. It is certainly one of the most interesting communications on this subject that has appeared for many years. The author will, we think, completely revolutionise craniology. He proposes a new method of classification based on the curvature of the base of the skull, which is highly interesting.

A new Caïman.—A new reptile of this kind has been discovered by M. Preudhomme de Borre in a collection purchased by the National Museum of Belgium.

POPULAR SCIENCE REVIEW.

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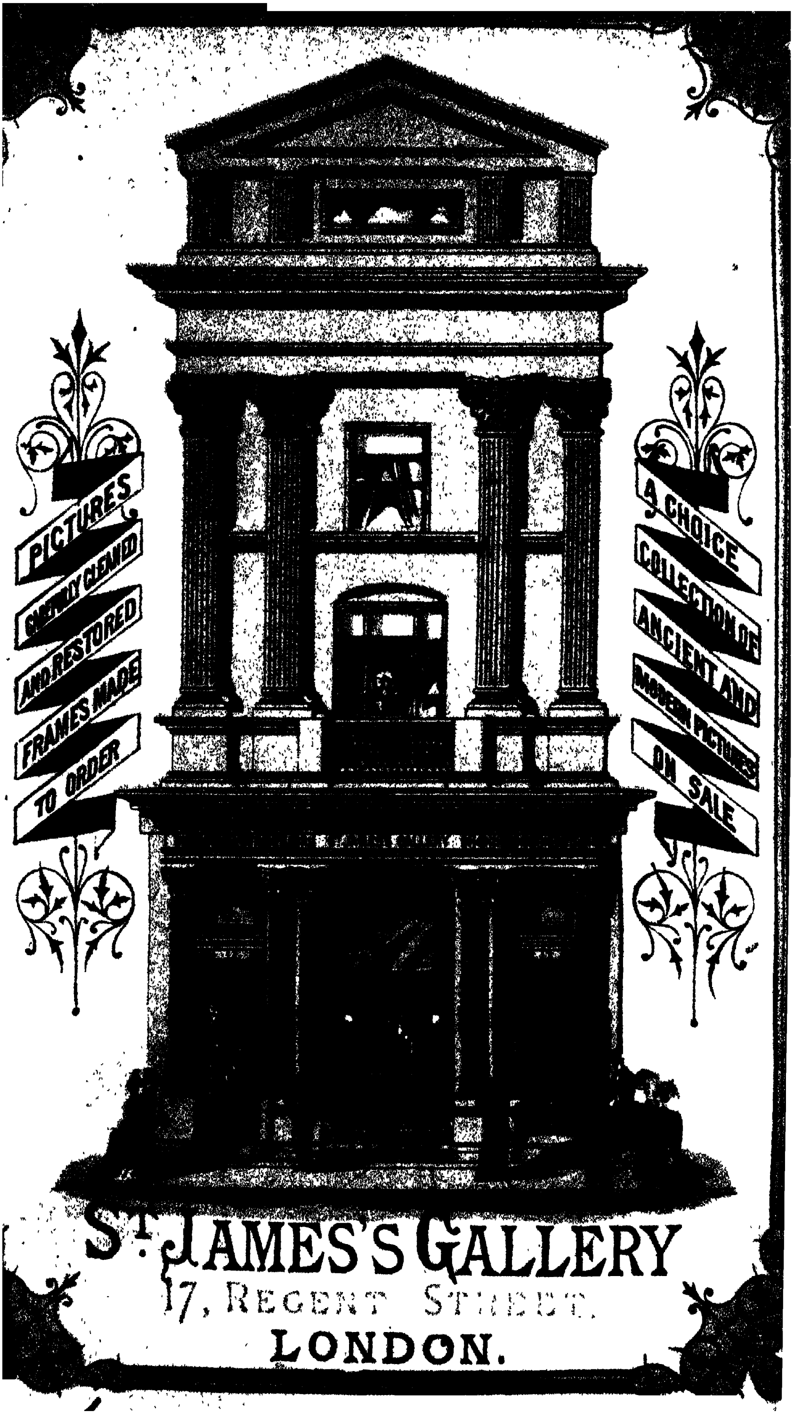
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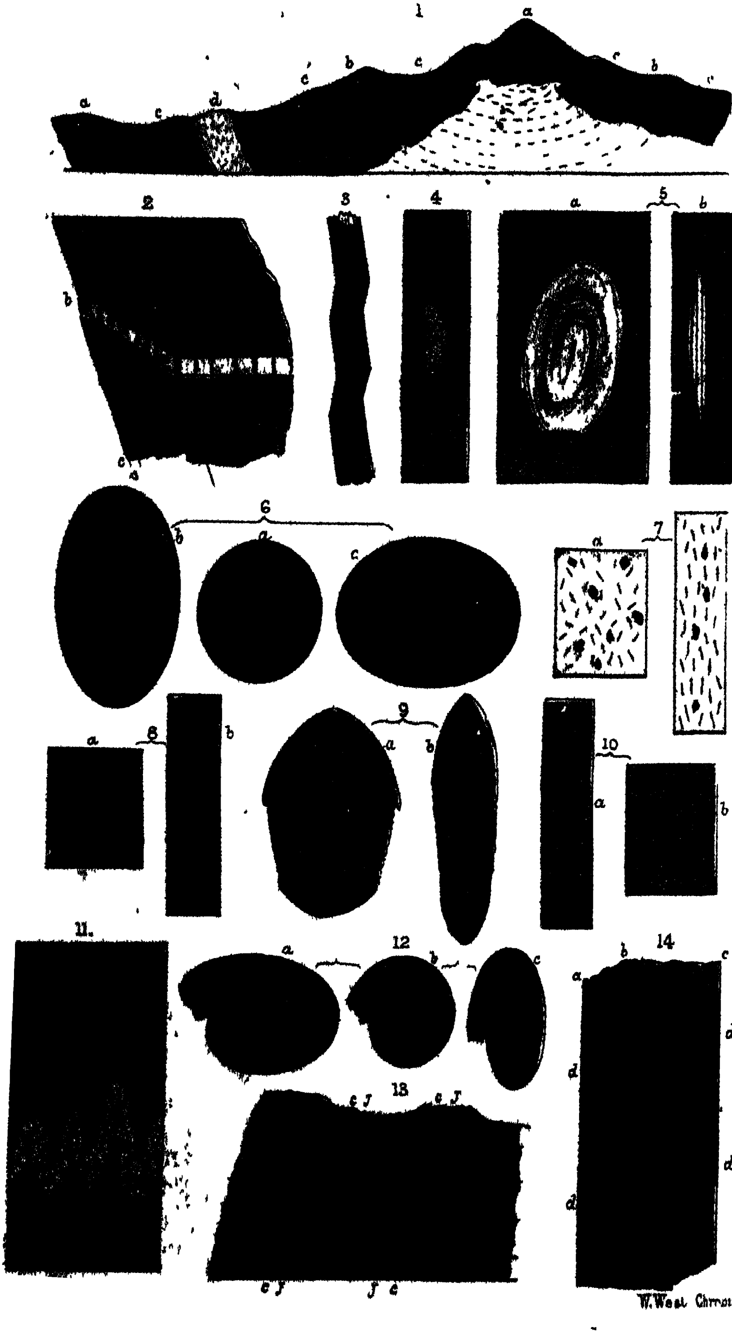
Plate LVII.



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THE STRUCTURE OF ROCK MASSES (STRATIFICATION, JOINTS, CLEAVAGE).

By DAVID FORBES, F.R.S., &c.

[PLATE LVII.]

IT appears extremely probable that one of the first observations which the beginner in Geology or Physical Geography would make, upon commencing in the field the study of the rock masses which compose the exterior of our globe, would be to notice that rocks are not mere amorphous or structureless aggregations of mineral matter, disposed at random like rubbish shot out of a cart, but that, as a rule, they possess some definite structure or internal mechanical arrangement of the particles of which they are built up; a little further observation will then point out that such an internal arrangement may, frequently at least, be altogether independent of the external configuration of the rock masses themselves.

It will next be perceived, that such rock structure may differ greatly in character in the different classes of rocks, and the conclusion will assert itself that, of all the varieties of structure met with in the field, those in which the particles show an arrangement in more or less parallel lines along which the rock can usually be more easily divided or split up than in other directions, are by far the most frequent in occurrence.

A still minuter consideration of parallel structure teaches that it may be classified under five distinct heads, to which the terms stratification, joints, cleavage, foliation, and striation *

* The term "striation" or "vitreous striation" is here employed to signify the parallel arrangement which is entirely due to the development of what are called the "striæ" of fusion, such as are seen in glass, furnace slags, obsidian, vitreous lavas, glacial ice, &c. This structure owes its origin to the effects of the (unequal rate of?) movement in the different layers of a substance whilst in a more or less viscid condition, in the act of consolidating from a state of liquidity.

have been respectively applied; the first and third of these, stratification and cleavage, are only encountered in rocks of sedimentary origin; the second, joints, although common to all rocks, whether sedimentary or eruptive, is, it must be remarked, not always, or even necessarily, a parallel structure, yet it is so often such, that, in order to avoid confusion, it becomes necessary to consider it also under this head; the fourth, foliation, is specially characteristic of the so-called metamorphic schists, and likewise occurs in certain eruptive rocks; whilst the fifth or last, striation, is entirely confined to such rocks as have solidified from a previous condition of liquidity or fusion.

Since to attempt more would be to exceed the space at disposal in a single number of this Review, it is proposed upon the present occasion to treat only of the three first of the above-named rock structures, namely, stratification, joints, and cleavage.

Stratification, or, as it is very commonly called, bedding, is the term employed by geologists to denote a parallel structure in rocks caused by the successive subaqueous deposition of layers more or less thick of mineral matter previously held in solution or suspension in water, and it might even be said in air, for, upon a smaller scale, and in more exceptional instances, stratification may be of subaerial origin, as for example, in the cases of beds frequent in volcanic countries, consisting of alternating layers or beds of volcanic ashes, sand, and still coarser scoriæ, which, after having been thrown up from the crater high up into the air, are spread out by the action of the winds over the neighbouring country, often to a considerable distance; occasionally also, the action of the winds upon loose surface sands, like those in the desert, produces an arrangement of the particles analogous to subaqueous stratification.

Although the planes of stratification are usually spoken of as parallel, this is not strictly true, for in reality they always have a gentle slope upwards from the sea towards that part of the land from which, by the action mainly of river currents, they have derived the rock débris, out of which the strata themselves are formed. Sedimentary beds are very frequently found to die out or thin out like wedges, which sometimes is owing to a deficient supply of like material, but probably more often to the disturbing effects of currents, &c.

Regarded on the large scale, however, stratification possesses all the general features of parallelism.

It not unfrequently happens, particularly in the case of sandstones, that, independent of the usual parallelism of the beds of deposition to one another, another series of more or less parallel lines may be seen developed within the particular beds themselves, and covering them diagonally at a greater or less incli-

ation to the true plane of sedimentary deposition: this is commonly called "cross or false" bedding, and is usually ascribed to the disturbing action of currents during the period of the formation of the strata; what has by Sorby been called "ripple drift" is an analogous structure. In the same mass of rock such false bedding may sometimes be seen inclined in opposite directions, in beds superposed one above another, a result probably due to alterations in the direction of the current in the same locality, such as the ebb and flow of the tides.

In most cases the planes of stratification in large rock masses can be traced with ease, owing to the occurrence of alternating beds differing in mineral character, such as those of argillaceous, arenaceous, or calcareous nature; when fossiliferous, the organic remains are a sure guide, since they naturally rest upon the planes of bedding which formerly were the sea bottom, and when such anciently formed the margin or shores of seas or lakes, it is not uncommon to find ripple marks, foot-tracks of animals, marks after rain drops, and cracks formed by the heat of the sun, causing the mud to contract in drying between the reflux and influx of the tides, all of which are so many proofs of the origin of this structure. Certain strata, such as the limestones, the chalk with its flint nodules and bands, the infusorial siliceous beds, and some of the iron-stone beds, have assumed the form of sedimentary strata, not by the mere mechanical deposition of mineral matter held temporally in suspension in the water, but by the agency of organic life in extracting the lime, silica, or iron, previously held in a different state of chemical combination and solution in sea or fresh water: abundant examples of this action still going on before our eyes are seen in the formation of the great coral reefs, the Atlantic mud, which is but recent chalk, and the lake iron-stones, which last have only lately attracted any attention, and are as yet very imperfectly described.

Other strata, as for instance the beds of rock salt, gypsum, and anhydrite, have no doubt been originally formed simply by the evaporation of inland seas or lagoons of salt water, previously holding these substances in solution.

In some rocks, more especially those pertaining to the older formations, in which, as yet, few or no fossil remains have been found, it is often a matter of considerable difficulty to determine correctly the lines of original sedimentary deposition. In such cases, dependence must be placed mainly on the observation of such minor differences in mineral character and structure as usually present themselves, even in the thicker masses of sedimentary rocks, and in the case of extremely homogeneous deposits, such as, for example, some of the great beds of clay-

slate, on the occurrence of coloured zones or bands of differently tinted stone.

Although, as before mentioned, all sedimentary strata have originally been deposited in horizontal or nearly horizontal beds, this uniformity of arrangement has, in the majority of cases, soon been broken up by dislocations, depressions, or elevations, caused by various disturbing influences; it is, however, beyond the scope of the present communication, to enter more into detail as regards abnormal stratification, since the present remarks are more exclusively directed to the consideration of stratification as a parallel structure only.

Joints.—In all the rocks, no matter of what geological age, or whether they are stratified or unstratified, there will be observed numerous lines of fracture, cutting through the rock mass in all directions, and at angles differing more or less from one another. Such divisional planes cannot, as a rule, be said to be parallel to one another, yet it is, at the same time, so often found that natural joints traverse even considerable areas as a series of straight and well-defined parallel lines, that it becomes imperative that this structure should be here taken into consideration, to avoid confounding such jointing with the parallelism of either stratification or cleavage, for numerous examples might be cited in which able geologists have even in the present day fallen into such errors.

In distinguishing parallel joints from all other varieties of parallel rock structure, it is most essential to remember that joints either are, or at one time must have been, mere true fractures or fissures; these cracks may at the present time still remain open, but they also may have become cemented together again, by infiltration or other causes, in some instances so firmly that the rock may now appear equally as solid as it was originally before the formation of such fissures; in which event, however, the difference in appearance of the cementing material usually indicates where they had originally been.

As joints, therefore, are mere lines of fracture, it naturally follows that the mass of rock enclosed between two such joints cannot, as is the case in both stratification and cleavage, possess in itself any structure enabling it to split up parallel to the sides of the joints themselves, yet in some cases there may still remain a doubt as to whether such a structure belongs to the class of joints, bedding, or cleavage. A somewhat more extended examination of the neighbouring rocks will generally solve the question; the parallelism of joints, however, is seldom or ever found to be more than a mere local occurrence.

Joints may be divided into two classes: first, those which owe their origin to purely mechanical agency, as in the case of those accompanying the dislocation, elevation, or depression of

land; when a force operates in elevating or depressing any large and rigid mass of rock, it must necessarily tend to develop a series of fissures or cracks, more or less regular in proportion as the force itself has acted with more or less uniform intensity over the area in question.

In general, the parallelism of joints is somewhat deceptive, as the lines of fracture are frequently in reality only radii, yet at the same time, this divergence may be but little observable, owing to the great length and number of the radiating fractures.

Although such joints may occasionally possess a considerable degree of regularity or parallelism over a somewhat extensive area, still this is rather the exception than the rule, as it is much more usual to see them developed in several directions at the same time, and at the most discordant angles to one another, so that the rock mass may be thus cut up into blocks more or less symmetrical, owing to the crossing of the different systems of joints, which are not necessarily of the same geological age.

The second class of joints is confined entirely to eruptive rocks, being due to the contraction experienced by such rocks when cooling: these can commonly be seen well developed in most quarries of hard stone, such as granite; here it is found that the direction of the joints follows in great measure, or is parallel to, the external configuration of the rock mass, which causes the formation of a series of more or less thick layers of separation (or benches, as they are frequently called), in some cases resembling true stratification, and not unfrequently thought by geologists, not well up in the study of such crystalline rocks, to be so many proofs of the sedimentary origin of such rocks.

As the cooling of such a mass of eruptive rock proceeds from the exterior inwards, layer after layer of the consolidated rock separates itself from the main mass below it by the effects of contraction, and from the same cause another set of vertical joints becomes developed in these layers at right angles to the first series, which tend to break them up, in turn, into more or less regular blocks.

In some of the more fine-grained volcanic lavas, basalts, phonolites, &c., similar parallel jointing may occasionally be found present, so numerous and close to one another as to give quite the appearance of lamination, and in some cases even to permit of the rock being split along them into tolerably thin plates.

Considered from an economic point of view, both these classes of joints are of the greatest importance; for it is only by taking advantage of their occurrence, that many massive rocks, such as granite, syenite, porphyrite, &c., which otherwise possess no divisional planes, can be quarried with facility. In mining ope-

rations also the presence of such joints greatly assists the progress of the miner in his subterranean explorations, besides reducing the expense of driving his galleries and shafts, which would be found to cost vastly more in the event of the rock being a solid and compact mass, devoid of such natural fissures.

Cleavage.—Although this structure had been noticed as far back as 1820 by Otley and Bakewell, the latter of whom pointed out that previous observers in the Alps had, in more than one instance, confounded cleavage with stratification, it seems quite to have escaped the attention of foreign geologists, and it was not until 1835 that, in his valuable "Essay on the Structure of large Mineral Masses," the true character of cleavage structure and its relations to jointing and stratification was clearly established by Professor Sedgwick, who first pointed out that the lines of cleavage are often continuous over long ranges of country, even across bent and contorted strata, and that they were quite independent of those of stratification.

In order to comprehend the phenomena of cleavage, it is absolutely necessary, in the first place, to understand its relations to stratification and jointing, which is best done by appealing to the eye. In fig. 13, Pl. LVII. a sketch is given, taken from Murchison's Silurian system, which shows all these three structures presenting themselves in the same quarry; we have, in the first place, a number of sedimentary beds of rock (*b b b b*) superposed one upon the other, which are traversed by a series of parallel joints (*j j j j*), whilst at the same time the beds are seen to be cut through at a totally different angle, by another, and much more numerous, series of perfectly parallel divisional planes (*c c c c*), which is the cleavage.

It is not at all difficult, however, to procure even hand specimens which illustrate the above relations in the most clear manner; in fig. 14, Pl. LVII. is seen figured a fragment of ordinary purple roofing slate of the cambrian formation, from the quarries of Mr. Assheton Smith at Llanberris in North Wales. In this small fragment, which is only about four inches long by two inches broad and one inch in thickness, the front face and its corresponding one behind are due to cleavage, whilst the side faces are bounded by two smooth parallel joints, which thus divide the slate rock into lozenge-shaped fragments; the stratification, on the contrary, is represented by the alternating bands of greenish and purple-coloured slate, the direction and angle of which are seen at a glance to be quite unconnected with those of either the cleavage or jointing; the specimen can be split *ad infinitum* parallel to the front face, i.e. that of cleavage, but possesses no divisional planes between the two joints, nor can it be parted along the coloured bands or lines of original stratification, which, in uncleaved slate, would have been the

lines of greatest weakness, but, in this case, as is usual in all good roofing slate, the tendency to separate or split along the lines of bedding has been altogether obliterated by the effects of cleavage.

The section, fig. 1, Pl. LVII. (Carnarvonshire), serves to indicate the mutual relations of the planes of cleavage and stratification on the large scale. The beds of clay slate, *b b*, with the intercalated grits *d*, are disposed in a curve resting on the one side against a mass of quartz porphyrite, the boss of the mountain being also composed of porphyrite, with however, little or no visible quartz: the cleavage of the slate is indicated by the lines *c c*, and is developed at a high angle across the stratification. The cleavage planes in North Wales are very commonly found to be at angles of from 30° to 40° with that of the stratification.

The development of the cleavage planes, and consequently the facility with which such cleaved rock will split up into slates, is entirely dependant upon the mineral nature of the original beds, for it must be recollected that cleavage is invariably a superinduced structure. When the beds are of a coarse character, the cleavage may not be developed at all, but it becomes more and more perfect in proportion as the rock constituents are themselves more fine and homogeneous; when alternating beds of grit and slate occur, the cleavage, which may be perfect in the latter, is at most very imperfectly developed in the former. When, as depicted in fig. 2, Pl. LVII., a thin bed of hard quartzose character intervenes between two beds of cleaved slate rock, the cleavage lines are stopped by this bed, but recommence on the other side of it, whilst, at the same time, the hard bed is usually more or less fractured by fine joints, which do not follow the angle of the cleavage, but are, in most cases, nearly perpendicular to the plane of the bed itself.*

A somewhat analogous effect takes place when the slate rock is composed of alternating thick and thin beds, all of slate rock, but differing considerably in hardness; in this case, the cleavage cuts through the whole, but instead of, as usual, following a perfectly straight line, it bends in and out at the junction of the soft and harder beds, so that a slate split of such rock may, in the length of a foot, show some six or eight bends in and out, like a sort of wave line, which commonly is rendered still more striking to the eye by being alternately tinted of a greenish and

* When cleavage lines cut through beds on the surface of which ripple marks exist, this structure is curiously affected, being distorted so that the ripples are compressed in one direction, and elevated like ridges or waves in the other. The attempt made to show this structure in fig. 2 is, unfortunately, not a success.

purple or slate colour; a feeble attempt is made in fig. 3, Pl. LVII. to depict the edge of such a slate.

In the case of joints, it is found that they traverse all rocks without any apparent reference to their hardness, and that they frequently divide cleanly, as if cut with a knife, even the hardest pebbles, which may, perchance, be in their way; with cleavage, however, it is different, for when the lines of cleavage meet bodies which oppose a resistance too great to be overcome, they stop short and recommence on the other side. A very beautiful illustration is represented in fig. 4, which shows an ideal section of a hand specimen in the possession of Mr. James Wyatt; this consists of a small block of cambrian slate from the celebrated Penrhyn quarry, near Bangor: the centre of this block contains a rounded pebble of hard diabase, about the size of a turkey's egg, which has been disclosed and loosened by carefully splitting up the block into thin slates, so that the pebble may be replaced in its position as before, with the slates around it so naturally, that its existence would not even be suspected. The cleavage planes, although interrupted by the pebble, are nevertheless continuous around it on all sides.

When, however, such foreign bodies are not sufficiently hard to resist the effects of cleavage, they are seen, as in the case of the yellow concretions so common in the purple slates of North Wales, to be compressed and elongated in the direction of the planes of cleavage, which pass uninterruptedly through them, quite ignoring their presence, as is shown in fig. 5, Pl. LVII. It has further been observed, that when hard substances, incapable of receiving cleavage structure, occur in cleaved rocks, that they are, as a rule, arranged with their long axis parallel to the planes of cleavage, as seen in fig. 4, Pl. LVII. This is a very common occurrence, and a woodcut, page 145 of Professor Ramsay's report on the geology of North Wales, shows how in the slate conglomerate near Lynn Padarn, the slate pebbles are all arranged with their major axis nearly upright along the lines of cleavage, instead of having their flat sides corresponding to the planes of stratification, as under ordinary circumstances they would naturally lie.

After that the labours of Professor Sedgwick had proved conclusively that cleavage and stratification were two distinct structures, much attention was directed to their mutual relations. Horizontal cleavage is not of common occurrence, but has been observed in Wales, Cornwall, and Devon; the angle or dip of the cleavage is usually much higher than that of the bedding, and the two are seldom found coincident except in cases of nearly vertical stratification. The lines of cleavage and stratification do not even necessarily dip in the same direction, for, as pointed out by Sedgwick, the cleavage planes in the gorge of

the river Wye cut across the anticlinal of the strata, and consequently cut across the same beds in two different directions, i.e. dipping both with and against the underlay of the beds.

That the inclination or dip of the cleavage planes varies infinitely more than the strike is quite certain, and observation shows that, in many cases at least, the cleavage planes on each side of an anticlinal may dip inwards towards the axis of elevation, consequently in opposite directions, so as to present a fan-like arrangement; this was specially noticed by Darwin in South America, and subsequently by other geologists. The late Mr. D. Sharpe attempted to generalise on these facts, and brought forward the hypothesis that the strike of the cleavage, when studied on the large scale, followed a general direction, but that the varying lines of dip formed a series of anticlinal and synclinal "systems of cleavage," extending over vast areas like a series of immense arches. It is to be feared, however, that subsequent researches in the field have not corroborated this hypothesis.

From the direction of the strike of the cleavage in some of the districts first examined it was at once taken for granted that the normal direction of cleavage planes would be exactly east and west of the magnetic north, and this gave rise to numerous attempts to account for this structure by the action of magnetic or electric currents; it required, however, but a little more extended observation in the field to prove this generalisation unfounded. Sedgwick also announced that "the strike of the cleavage is nearly coincident with the strike of the beds," which is, no doubt, often the case in districts in which the direction of the beds is tolerably uniform, but quite at fault when the stratification is of a more complicated and contorted character. Professor Phillips, in 1843, arrived at the conclusion with respect to the slate rocks of Wales, that the cleavage planes are always parallel to the "main direction of the great anticlinal axes, but are not affected by the small undulations or contortions," and this conclusion appears to hold good in the majority of cases; yet exceptions are not wanting, as Professor Phillips has himself pointed out.

In summing up the evidence it can only be stated, that it appears certain that there is a decided tendency in the cleavage planes to follow, more or less, the strike of the strata or axis of elevation. Yet, at the same time, the inclination of the cleavage planes does not appear to have any connection with the dip of the stratification, and it appears most in accordance with facts to assume, that both the strike and dip of the cleavage planes are entirely governed by local circumstances. Since cleavage is not known to occur in normal sedimentary rocks, it is evidently dependent upon the local disturbance of the strata,

and most probably intimately connected with the intrusion of the eruptive rocks of the district.

The causes which are concerned in the production of cleavage structure were for a long period involved in obscurity. Some of the early observers attributed these phenomena to concretionary, crystalline, or chemical action, and others to the effect of magnetism or electricity; as, however, the explanations given were both vague and unsatisfactory, besides not always consistent with the supposition that the expounders of these hypotheses were themselves altogether at home in the study of those forces which they thus invoked to their aid, it is to be suspected that the hypotheses advanced were but another mode of accounting for ignorance.

It was quite evident, however, that crystallisation could not be the cause, since, in all cases of perfect cleavage, no trace of crystallisation can be detected even with the microscope, and in cases where crystalline structure is developed, the cleavage is invariably much more imperfect, or altogether absent.

The late Mr. Daniel Sharpe was, however, the first who pointed out the true road to the solution of this problem, when he announced, as the result of his observations, that cleaved rocks had undergone a compression of their mass in a direction perpendicular to the cleavage planes, and showed that in cleaved rocks the particles were always arranged with their flat sides parallel to the cleavage planes, and that they were usually longer on the line of dip of the cleavage than on the strike. He also confirmed what had previously been noticed by Professor Phillips, that the fossils, when present in cleaved rocks, were always more or less distorted, the distortion being greatest where the angle between the cleavage and stratification is least.

The microscopic examination of sections of cleaved and uncleaved slate rock showed their structure to be very dissimilar. In the latter, as seen in fig. 8 *a*, Pl. LVII., the grains of sand, clay, mica, &c., which compose the mass of the rock, are seen as if disposed at random, without any trace of definite arrangement, whilst fig. 8 *b*, which shows a section of cleaved slate (roofing slate), proves the particles to be distinctly arranged and elongated along the line of cleavage of the slate.

Sorby also showed, as figured in fig. 10 *a* and *b*, Pl. LVII., that analogous differences in structure occur in the cleaved and uncleaved devonian limestones from Devonshire, the latter being a section of an encrinite joint, in which the cells are arranged without any order, and retain their normal or equiaxial shapes. In the former, however, which is a highly cleaved limestone from Kingskerswell, the structure is seen to be entirely changed,

the cells being in this case flattened and drawn out, so that the longer axis lies in the line of the cleavage.

In the same manner the yellow concretions, which look like so many blotches in the Welsh slates, when found in uncleaved slate rock, are seen to be nearly spherical or ellipsoidal concretions occurring in the lines of stratification, and which, when they are longer in one direction than another, invariably have their major axis lying in the plane of bedding. In the cleaved slate, however, they are found to be considerably changed, as seen in fig. 5 *a* and *b*, Pl. LVII., where they are represented as flattened out and compressed sidings; the major axis of these now always follows the direction of the lines of cleavage.

The peculiar effect of cleavage in altering the form of fossils, as has previously been noticed, has proved a puzzle to the palæontologists, who, deceived by appearances, have frequently given different names to distorted specimens of the same fossil. This was the case with the two names *Spirifer disjunctus* and *Sp. giganteus*, the latter being subsequently shown only to be the former greatly distorted and expanded by the effects of the cleavage in the slates at Tintagel in Cornwall. In like manner, the shell *Euomphalus pentangulatus*, from Little Island, Cork, represented by the figures 12 *a*, *b*, and *c*, Pl. LVII. (from Haughton), had frequently (owing to the distortion and consequent change of form from their normal appearance, fig. 12 *b*) been described as *Ellipsolithes*, and considered as quite distinct from *Euomphalus*; and several other quite similar mistakes could be mentioned.

Another example of how greatly the appearance of the same fossils, when occurring in cleaved rocks, may differ from one another will be seen in the case of the trilobite *Asaphus Homfrayi* (Salter), shown in fig. 9 *a b*, Pl. LVII.

A comparison of the normal and distorted fossils proves, without doubt, that they must have not only been subjected to a compressing action, but also that this has at the same time been combined with a sort of sliding movement, which has caused them to become drawn out or elongated along the planes of cleavage in one direction much more than the other. As an experimental illustration of how such an effect can be produced, the changes which a coin, in this case a farthing, will experience when passed through a rolling-mill, are shown in fig. 6 *a*, *b*, *c*, Pl. LVII., and which will not require any explanation. The action of the rolls upon a substance inserted between them is a combination of direct pressure with forward movement, and consequently not only causes the raised impression on the coin to become flattened out, but also at the same time elongates its one axis in the direction of the forward movement, i.e. that in which it had been inserted between the rolls, whilst

the other axis is but little expanded; by this means distorted images like *b* and *c* can be produced at pleasure, merely by passing the coin through the rolls in one or other direction.

The idea that an approximate measurement of the amount of compression to which cleaved rocks had been subjected might be deduced from calculations based upon the relative dimensions of normal and distorted fossils, naturally suggested itself, and has been followed up by several observers, particularly by Professor Haughton, whose conclusions are strikingly corroborative of those obtained by other experimenters by totally different modes of procedure, as will subsequently be alluded to.

The researches of Mr. Sorby, however, not only advanced the previous knowledge of the phenomena of cleavage structure, but proved, experimentally and conclusively, that cleavage was not in any way connected with chemical, crystalline, or electrical agencies, and that it was solely the effect of mechanical forces acting upon the rock so as not only to effect a considerable compression or condensation of its substance, but at the same time to re-arrange or change the position of the particles of which the rock is composed.

One of the most interesting illustrations brought forward by Mr. Sorby to show the effect of such compression, is seen in fig. 11, Pl. LVII., being a vertical section from near Ilfracombe, where a bed of very much contorted coarser arenaceous slate is seen interposed between two beds of fine-grained well-cleaved slate, the stratification lines of which are denoted by the darker bands—*a*, *b*. An inspection of this section at once renders it apparent that these beds, originally deposited as horizontal strata, must have been squeezed together from the sides, so as to crumple up the non-yielding arenaceous bed, whilst the particles of the slate, giving way to the compression, were only packed together more closely, and became so much denser than before. To use the author's words, these phenomena are "analogous to what would occur if a strip of paper, for instance, was included in a mass of some soft, plastic material, which would readily change its dimensions. If the whole was then compressed in the direction of the length of the strip of paper, it would be bent and puckered up into contortions, whilst the plastic material would change its dimensions without such being the case; and the difference in distance of the ends of the paper, as measured in direct line or along it, would indicate the change in dimensions of the plastic material."

Numerous observations, some of which have been already referred to, prove that the arrangement of the particles in cleaved rock differs from what it is in normal sedimentary rocks, and that in the former the particles composing them

have, as a rule, their flat sides or long axes parallel to the planes of cleavage, and not to those of stratification, as is the case in unaltered sedimentary deposits. That this effect was due to the same mechanical force which produced the cleavage, was demonstrated by Mr. Sorby by the following simple experiment:—

A quantity of scales of oxide of iron was mixed promiscuously, so as to distribute them in all directions throughout pipe-clay; after drying, a section of the mass presented the appearance shown in fig. 7 *a*, Pl. LVII. Upon submitting this mass to a pressure sufficient to change its dimensions to the same extent that is supposed to have taken place in natural slate rocks, a section at right angles to the diameter of the applied pressure (which, consequently, would correspond to the dip of the cleavage planes in roofing slate) now presented the appearance seen in fig. 7 *b*, rendering it apparent that the effect of the pressure had caused the scales to re-arrange themselves in nearly parallel lines, perpendicular to the pressure, along which the mass now admitted of easy division into thin plates, whereas it did not do so in other directions.

Ordinary clay, which contains but few flat particles, when submitted to pressure was also found to receive a most distinct laminated structure, but did not cleave perfectly, so that it is probable that the perfection of the cleavage in roofing slates depends, in part at least, upon the presence of the numerous flat particles of mica, chlorite, &c., which, by their re-arrangement, also contribute to determine the easy splitting qualities of the rock. That rocks may receive a comparatively well-developed cleavage structure, even in the absence of such particles, is evident, however, from the descriptions given by Sorby of the cleaved limestones of Devonshire; and the subsequent experiments of Tyndall showed also that a most perfect cleavage could be induced in pure white wax, by the application of mere pressure.

The consideration of the above, and numerous similar data connected with the subject, led Mr. Sorby to the conclusion that rocks possessing a perfect cleavage structure, such as the Welsh roofing slate, had experienced a diminution in bulk by compression to about one-half of their original volume (consequently would, after cleavage, increase so much more in density), and to advocate a purely mechanical theory of cleavage, such as explained above.

EXPLANATION OF PLATE LVII.

FIG. 1. Sketch section across Llwyd-Mawr, Carnarvonshire; (*a*) quartz porphyrite on which rests the purple cambrian slate rocks; (*d*)

Lingula grits, followed by blue slaty rocks of the Bala or Caradoc series, capped by (*a*), which is a boss of porphyrite without visible quartz; the lines *b* and *c* denote respectively the planes of stratification and cleavage.

FIG. 2. Diagram showing the relations of the cleavage lines when cutting across a broad but thin quartz bed; the upper plane is an attempt to represent the distortion of ripple marks occurring on a plane of stratification affected subsequently by cleavage, which renders them jagged and irregular by the compression they have undergone. A fine example of this may be seen on the roadside between Pont Aber Glaslyn and the Croesor Valley, North Wales.

FIG. 3. Shows the edge of a slate from Llanberris, North Wales, which, instead of being straight, has numerous bends, owing to the cleavage lines being deflected when passing through thin slate beds of unequal hardness.

FIG. 4. Ideal section of a slab of slate from the Penrhyn slate quarry at Bangor, containing a hard pebble of diabase inclosed in the cambrian slate: although interrupting the cleavage lines, its major axis lies parallel to them.

FIG. 5. Yellowish concretions in the purple cambrian slates of Llanberris, (*a*) in plan, (*b*) in section, showing how they lie flattened out in the line of cleavage and cut through by the cleavage planes.

FIG. 6. A farthing in its normal condition (*a*); ditto, after passing it longitudinally between the rolls (*b*); and the same, transversely (*c*), showing how the image becomes distorted lengthwise or crosswise, according to the direction in which it is rolled.

FIG. 7. Section of a mass made up of pipe-clay and smithy scales (*a*), and a section of the same edgeways, after being compressed to one-half its volume (*b*) (Sorby).

FIG. 8. Microscopic section of ordinary slate rock, (*a*) uncleaved, (*b*) cleaved.

FIG. 9. *Asaphus Homfrayi* (Salter), a trilobite from the Tremadoc beds, (*a*) but slightly flattened out, (*b*) distorted by cleavage.

FIG. 10. Sections of devonian encrinite limestone from Devonshire, (*a*) cleaved; (*b*) uncleaved (Sorby).

FIG. 11. Vertical section seen in the cliffs near Ilfracombe (Sorby), showing the crumpling up of hard arenaceous beds when they occur in well-cleaved slate.

FIG. 12. Figures (*a, b, c*) of normal *Euomphalus pentangulatus*, and the same distorted in two directions, from Little Island, co. Cork (Haughton).

FIG. 13. Diagram of a stone quarry, which shows the occurrence in one mass of rock of stratification (*b b*), cleavage (*c c*), and parallel joints (*jj*) (Murchison).

FIG. 14. Lozenge or rhomb-shaped fragment of cambrian purple slate from Llanberris, North Wales, showing the cleavage planes (*b c*), bounded by two smooth joints (*a b*), and the colour bands or stratification (*d d*), all in a hand specimen of about 4″ × 2″ × 1″ in dimensions.

THE PLANET JUPITER, 1869–1870.

By the Rev. T. W. WEBB, M.A., F.R.A.S.

THE singular appearances presented during the late season by the planet Jupiter have naturally received, as indeed they have deserved, much attention from astronomers; and in many instances no doubt from those possessed of optical appliances, as well as eyes, superior to my own. This consideration, however, has not induced me to withhold a series of observations which I undertook as far back as the middle of October; for every original contribution to our general store of astronomical knowledge may possess a certain, though it may be a very subordinate value. As regards astronomical phenomena in general, a comparison of descriptions and representations by various eyes and hands, and in the use of different telescopes, points very distinctly to the conclusion, that where so much of personal and instrumental equation exists, a collection of independent details from any fairly reliable quarters may not only be permitted but recommended: in such an accumulation, though error must necessarily be involved, there will be a continual increase of the elements of truth. The readers of the present paper will therefore be content to take my statements as they find them. I have attempted to describe simply what I have seen; not doubting that others may have seen more, and understood it better.

An important distinction has been repeatedly pointed out, between the group of interior and exterior planets, as referred to the wide interval now known to be occupied by a multitude of minuter bodies. Either group, as far as observation extends, or fair analogy will carry us, has a character peculiarly its own; the outer being distinguished from the inner by inferior comparative density, but superior magnitude, velocity of rotation, and attendance of satellites. These remarkable differences, though increasing the interest of such researches as may be permitted to us by the Great Ruler of the universe, add materially to their difficulty; and we find it impossible to carry on to remoter planets the analogies which have apparently served

us so efficiently in the case of our closer neighbour Mars. It is fortunate, therefore, for the purpose of our study, that Jupiter, the nearest at once and the largest of that external group, presents a disc so broad and so luminous as to invite examination even with telescopes of moderate size; and it may be hoped that a persevering scrutiny will produce some interesting result in the end.

The instrument employed in the following observations is a Newtonian silver-on-glass reflector, with a speculum by With, of $9\frac{1}{3}$ inches aperture. It is capable of separating γ^2 Andromedæ and μ^2 Bootis, and showing steadily the minute *comes* of μ Andromedæ; and its definition of Jupiter with a power of about 212 was always as satisfactory as the state of the air would permit; the latter, I regret to add, was frequently such as to offer great and occasionally insuperable obstacles. It was very seldom that I attempted to employ a power of 450, but even when its definition was tolerable, the light was too much diminished to render it of service. I had no micrometer available for such minute details, but trusted to the estimate of a tolerably experienced eye, accustomed to drawing as well as to observation. The series was commenced on Oct. 15, 1869, and continued till March 11, 1870; during which interval the planet was examined, though sometimes to little purpose, on forty nights.

In order to render description more ready and intelligible, I have thought it allowable to assign, for my own purpose, and for the present season, names to the different features of the planet. I have called the brighter stripes, *zones*, the darker, *belts*. The central portion, which has recently exhibited such singular details, I have termed the *equatorial zone*; its two dark borders, the *north* and *south torrid belts*; the two bright regions on either side of these, the *north* and *south temperate zones*, the latter being subdivided by a dusky stripe, which may be called the *south subtorrid belt*; beyond these two zones respectively lie the *north* and *south temperate belts*; and on the further side of these, the two *polar regions*. The analogy of which I have availed myself makes of course no pretension to accuracy; but it will answer for present identification, if not for future recognition. A reference to the figures on the next page will exhibit these features to the eye.

We shall now attempt a slight description of these regions in order, beginning from the centre of the disc.

The *Equatorial Zone*, with the two Torrid Belts that enclose it. These boundaries were always continuous, uniform, and of a grey tint, which probably under more favourable circumstances might have been distinguished as "deepened yellow or yellow-grey, or faint chocolate;" such, at least, is the note

of their hue on occasion of the fine display on Nov. 16; and I thought them purplish on the following night. For variations in depth of shade, or breadth, no standard of comparison existed except the north temperate belt, which certainly wore a more uniform aspect; assuming the absence of change here, it was frequently perceptible in the torrid belts. For instance, on Dec. 11, I have noted that they were both much less conspicuous than formerly, and only about half the breadth and darkness of the north temperate belt; but in the next observation, two nights afterwards, the contrast was not so striking: subsequently, that belt is frequently recorded as stronger and darker than either of them, though without so striking a difference except on Jan. 5 and Jan. 12, when the recurrence of that proportion is expressly mentioned. These changes of aspect were at times very rapid; for instance, on Dec. 25, at 8h. G.M.T. the north temperate and the two torrid belts were for once equal, but at 9h. 45m. the latter had recovered its accustomed deeper tone. In such apparent variations it is not always easy

Fig. 1.

Nov. 16d. 10h. 35m.

Fig. 2.

Nov. 17d. 10h. 35m.

to distinguish between the optical effect due to rotation and the results of actual local disturbance; in many cases both may have been combined: a remark which may be extended to every portion of the visible hemisphere. On Feb. 25, for the first time, these two belts were noticed to be relatively unequal, the south being considerably darker than the north at 6h. 55m. and 7h. 45m., but at 8h. 15m. the difference had become but slight; and on the following evening at 7h. 20m. both were equally feeble. March 5 and March 11 the south was again rather the darker. The only estimate I find of their apparent dimensions was on Feb. 12, when I thought that the two together amounted to more than $\frac{2}{5}$ but less than $\frac{1}{2}$ of the whole central region.

The intervening space, which we regard as the *Equatorial Zone*, has been the scene of the phenomena which have attracted such general attention. In the first place its colour has been so marked that it would force itself upon the notice of even a careless observer. I saw it Feb. 5, with as low a power as 110.

There has been some difference in the estimate of its tone, which might indeed be expected, since eyes are known to differ widely in this respect—more, in fact, than instruments: for the same blue rays which are "outstanding" or external to the image, in achromatics, are, partially at least, transmitted instead of being reflected by the silver film; so that the result in either case will be similar—the predominance of the complementary orange in the focal image. But whatever might be due to personal or instrumental equation, the contrast of the central band with the rest of the disc was most obvious. To my eye the colour appeared to be a ruddy or brownish-yellow. I had an impression towards the end of the year that it was becoming less decided; but this idea was subsequently shaken, and the suspected differences may have been due to the difficulty of such comparative estimates, or perhaps to actual inequality of tint in different parts of the zone.

Across this coloured background, a number of grey shadings were projected from the inner edge of the south torrid belt, usually at right angles to it, though sometimes a little deflected eastward, extending about half-way across the yellow zone. These, in a bad state of the air, could not be made out, or only appeared as confused and shapeless markings; under better definition it could be perceived that their further extremities were bent right and left so as to form a succession of loops or festoons, whose undulating edge usually ranged along the equator. From a rough graphical estimate there may have been sixteen or eighteen of these loops around the girdle of the planet, and they appeared to exist on every side of the globe, with a surprising general similarity of character, though not exclusive of local differences as to distinctness and symmetry of form. One would frequently surpass in breadth all the others in sight, and at times they were darker or better marked on one side of the disc than on the other. Dec. 25, they were shorter and narrower than before, but the usual type soon returned. In one very remarkable view (Nov. 16), six of them assumed the aspect of a bridge; the piers being very clearly made out in the centre, though more difficult towards either limb; and as they were always broader at their origin in the south belt, the resemblance to elliptical arches was very striking, notwithstanding some want of precise correspondence. A faint idea of this appearance may be obtained from fig. 1. On this occasion, though I have noted that there was "a great profusion of minute detail," which I had unfortunately not sufficient time to analyse or sketch, the festoon form was only partially and slightly indicated. But on the following night, Nov. 17, another side of the globe, about 150° removed from that previously visible, exhibited, though feebly through a sheet of streaky haze, a most

beautiful scene, very imperfectly depicted in fig. 2.* The southern portion of the equatorial zone was now so progressively toned down into shadow from the north as to give the impression of a hollow lighted obliquely in the contrary direction: the yellow spaces enclosed by elliptical arches above, and similarly shaped festoons below, being most luminous in their upper part, and being shaded off into the festoons beneath, received the opposite effect of actual convexity. The illusion was remarkable: solid ellipsoids seemed to stand out of, or be freely suspended in, a depressed channel; or it might be compared to a modification of the moulding known as "bead and hollow" in architecture—a broad concavity placed horizontally, studded along its upper half with longitudinal bosses almost like the backs of spoons, and illuminated with an oblique soft half-light. So singular was the deception, that it required an effort of the judgment to rectify the mistaken conviction of the sight: once impressed on the eye, it frequently returned on other nights, though never in the perfection of this display. The ovals were generally not very unequal in dimensions; though one would occasionally be brighter and better marked than the rest; in this instance a very minute one occupied part of a wide grey space. The following night, Nov. 18, when five rotations of the planet, except one hour, had been accomplished since the display of Nov. 16, the bridge had been replaced by the hollow moulding; four ovals were distinctly seen, though in inferior air; and there was a wide interstice a little east of the centre, in which no smaller yellow cloud was present, or the resemblance to Nov. 17, though on another portion of the circumference, would have been almost complete. As the planet increased its distance from the earth, these details, which at the best were of great delicacy, and interrupted by very slight atmospheric disturbances, became less and less distinct; even favourable air did not again unveil such striking scenes; but when anything could be made out, the elliptical areas were sure to return. In my earliest observations I had evidently seen, without clearly apprehending, this peculiar configuration; and it continued more or less uniformly to the close. At times some of the south edges of these areas distinguished themselves by their strong yellow or even whitish light; on one occasion (Nov. 15), I have recorded that these spaces, two of which were visible, of an "elliptical, or rather rounded quadrangular" form, encroached upon the south torrid belt to one-half of its thickness, and once only (Oct. 27), a grey interval seemed separated from the south torrid belt by a narrow brighter stripe,

* The shadings in these sketches are too dark; but it would be difficult otherwise to obtain sufficient effect.

which could hardly, from the rotation, have been identical with the little lengthened cloud in a similar situation on Nov. 17.

This equatorial region, comprising the yellow zone and its grey borders, lay a little south of the horizontal line bisecting the disc. I first noticed this on Nov. 6, the day previous to the opposition, and have almost always recognised it when my attention was directed to the point. My estimates, however, of the relative proportion of the breadth of this central region to that of the parts of the disc north and south of it, from which the amount of obliquity would follow, were not as accordant as I could have wished; but some allowance may be claimed in the case of such minute quantities. Nov. 19, assuming the breadth of the region $=1$, I considered the north segment of the disc $=1\frac{1}{2}$, the south $=1\frac{1}{4}$. I found, however, on referring to my previous sketches, that this must have been in error, and repeating the comparison on Dec. 11 and Dec. 15, I made the proportions north $=2\frac{1}{2}$, south $=2\frac{1}{4}$, and in these I long continued to acquiesce. Feb. 25, however, I felt doubtful as to the existence of any difference, either side appearing $=2\frac{1}{4}$ or $2\frac{1}{2}$. Feb. 26, the previous estimate returned. March 5, the planet from its west azimuth passing almost vertically through the field, I gave it south $=2$, north $=2\frac{1}{4}$; and a subsequent comparison of my various sketches led me to think that this must be nearer to the truth than $2\frac{1}{2}$ and $2\frac{1}{4}$, which makes the graphic projection of the central region too narrow for the eye. On March 10, I found the estimate of $2\frac{1}{4}+1+2$ confirmed; assuming this, therefore, to be "within blundering distance," it gives the value of the optical displacement of the centre about 0·095. That due to the inclination of the planet's axis + his longitude would be 0·070: from which we may infer that the appearance was not deceptive, and that the centre of the coloured band actually coincided with the equator of the planet.* The agreement, though of course but a very rough approximation, is the more satisfactory, as I was not aware, till towards the last, of the direction of the axis. I do not know that the obliquity of the planet's equator, as indicated by the unequal division of the disc, has been previously matter of direct observation. The belts had always been considered straight by me till March 10, when I fancied the equatorial region curved: an instance probably of the influence which previous knowledge has upon the eye.

We now leave this region of singular configuration, and pro-

* For this determination I am chiefly indebted to the kindness of Mr. Proctor. The direction of Jupiter's axis is not given in ordinary astronomical treatises.

ceed to the neighbouring districts, the north and south temperate zones.

The *North Temperate Zone* was the whitest and most luminous part of the disc, and, I believe, was quite constant in this respect. I never noted a speck upon it, though, Nov. 17, its north edge showed, I thought, a yellowish stain. Dec. 11, I estimated its breadth = $\frac{3}{4}$ of equatorial region.

The *South Temperate Zone* was more extensive than the corresponding north region: occasionally, at least half as broad again; but varying in this respect from the uncertain dimensions of the belt forming its south boundary. It contained no special markings, but was very liable to longitudinal subdivision, especially by a grey streak, which, for the occasion, I have termed

The *South Subtorrid Belt*.—This differed from those already specified, in not being continuous round the globe; whence I have preferred considering the zone in which it occurred, as a whole. It was a grey streak (purplish, I thought, as well as either torrid belt, Nov. 17), taking its origin abruptly close to the south torrid belt, but divided from it by a narrow bright interval: thence it diverged eastward at a slight angle of possibly 2° or 3° for a considerable distance, the same direction being traced, Jan. 12d. 9h. across the whole disc: subsequently it became parallel to its neighbour, with an intervening zone equalling or exceeding the breadth of the latter: in its further extension it became very faint and narrow, and it wholly expired (as far as my optical means would ascertain) before the west end returned in sight; so that occasionally the south temperate zone appeared unbroken. Jan. 5d. 8h. 55m. I suspected a renewed divergency before its extinction. Its aspect as to the width and darkness was very variable; and this obviously from actual local change, as well as from great inequality in different parts of its length: for example, Dec. 11d. 10h. 45m., while its commencement had crossed $\frac{5}{6}$ of the disc, it was equal to either of the torrid belts; and Dec. 28d. 9h. 30m. in a similar position it was nearly a match for its neighbour (those central belts were however then in a feeble condition): on the contrary, Dec. 25, when it traversed $\frac{2}{3}$ of the disc, it was faint, especially towards the east limb; and, in a position of parallelism to the nearest belt, it was sometimes broad and dark, at others narrow and feeble, and at very different distances from its companion. Its length must have been variable; Dec. 28d. 5h. 5m. it was extremely feeble and thin, and distant; 8h. 40m. its recommencement seemed to have already crossed $\frac{2}{3}$ of the disc, with a very evident divergence, so that there could have been then but little interval between the ends of the spiral: while, on many other occasions, the disc appeared clear from it. Dec. 15, I estimated the breadth of the two segments of the zone which this belt

divided as 2 to 3 (the broadest to south), the north temperate zone being rated 4. Dec. 21 and 27, these spaces ranged respectively as 1, 2, 3.

The *North Temperate Belt*, which we next take, and which from many drawings may be placed in latitude 25° or 30°, was in point of intensity the most conspicuous object on the disc, as well as one of the most constant. It grew, however, both in breadth and darkness, as time went on. Oct. 19, it was as dark as the north torrid belt, but narrower, and very distinct and defined—its unvarying characteristic. Nov. 15, it equalled its neighbour in breadth; latterly it was invariably darker, and frequently recorded as broader. In the fine view of Nov. 16, it was a very striking feature, of a very decided reddish-purple tint; darkest along its middle portion; and having close beneath it, east of the centre, two minute dark spots, not well made out. Jan. 25, there was again some suspicion of one or two dark projections. The purplish hue is frequently referred to: I thought it on several occasions less decided, but of course with uncertainty where there could be no direct comparison. Nov. 17, its south edge seemed bordered with yellow (as already noticed under the head of the adjacent belt). Jan. 5, when very broad, its south boundary seemed "fringed," or feeble: there is a similar note as to its edges in a parallel case on Jan. 25.

The *South Temperate Belt*, which lay in a higher corresponding latitude, and less in sight, than that in the north hemisphere, was superior at times to it in breadth, but in strength, definition, and permanency far inferior. It was usually a feeble object, sometimes, especially towards the last, very diffused and undecided; now and then with only one boundary, being a mere edge to the grey polar region; more commonly a separate belt; each of these aspects occurring on Dec. 28. Nov. 9, its ends were unequal in strength, and this was reversed in 2h. Nov. 15d. 9h. it was double; a fainter copy of the south torrid and subtorrid belts. 10h. 20m. the pair had so closed up that any separation was doubtful. Nov. 16d. 8h. 35m. it was closely double. 10h. 35m. for $\frac{1}{3}$ its length it was thinned off with a square break, as in fig. 1, its north edge continuing on, I thought, by a feeble line; about $\frac{2}{5}$ from the east limb it contained a very distinct longish white spot. Nov. 17, there were two feeble belts, as in fig. 2, which were not noted on subsequent nights. Jan. 25, it was broad and diffused, its edge trending obliquely *np* and *sf*; and where room was thus left, two narrow feeble belts crossed the east half of the disc, the furthest north of which came into the position of the subtorrid belt; occurring, however, in the open space between its two ends. The obliquity was again suspected, Feb. 25. Jan. 27, it was broad, with one or two white

intervals in it, as if it had been double, and connected by wide transverse bands; but too faint to be sketched.

The *North Polar Region*, or that which appeared so in the disc in consequence of foreshortening, occupied a large portion of that hemisphere. It exhibited an uniform dusky streakiness, where change, if it occurred, would be greatly masked in perspective. In the immediate neighbourhood, however, of the north temperate belt, frequent variations took place in a narrow luminous zone which usually divided it from the greyness of higher latitudes: sometimes this zone would be so darkened that the great north temperate belt would appear as the border of the dusky polar region; at others two narrow bright zones, the north usually the smaller, divided by a thin grey belt, would make a conspicuous separation there. This thin belt at times (Nov. 6, Feb. 25, March 11) assumed some importance as a humble companion to its grand neighbour; at others, from the dissipation of the zone on its north side, it would become the darker edging of the grey region; and sometimes it was hardly perceptible. These changes were no doubt partly due to local action; for instance, Feb. 25, the north of the two narrow zones had almost entirely faded in 1h. 20m.: but the sides of the globe may have been different that came successively into view.

This region was distinguished by another peculiarity—a feebler tinge of the same brownish-yellow so conspicuous in the central zone. I noticed this in the second observation of the series, on Oct. 19, when I fancied it did not reach the pole. Oct. 21, I imagined it more extensive; and subsequently, when attended to at all, it seems to have been diffused over everything beyond the north temperate belt. It was never more than a slight tinge; sometimes doubtful—especially of late; frequently quite distinct; the difference depending possibly upon the state of the weather or the eye.

The *South Polar Region*, being more removed from sight, and more compressed by its own temperate belt, possessed little worthy of notice. Nov. 16, there was a feeble dusky streak there, as in fig. 1. Nov. 17, fig. 2 shows a streaky grey. But, on the whole, it was characterised by being brighter than the opposite pole, and of a cool neutral grey without any tinge of yellow, excepting on Dec. 11, when 450 made it slightly tinted in comparison with the bright white of the north temperate zone.

A few general remarks may suitably close this series of observations, which cannot be said to have added to our present knowledge of the planet, but may possibly be of some little use in future comparisons.

Nothing was noticed tending to contravene the received opinion that the bright zones are of the nature of atmospheric

condensations, and that their form is determined by the velocity of rotation. The fact that the singular configurations of the central zone, though lying in the region of the greatest speed, showed but little trace of the general parallelism, may be explained on the supposition that they lay too deep for this disturbing force, which may naturally be most active towards the upper limit of the atmosphere. To this, however, it might be objected that, if darkness is to be held a test of depth, the north temperate belt, at no great distance, gave testimony of an action penetrating to a much lower region than that of those central markings, whose comparative quiescence thus remains unaccounted for; and the matter seems to require further investigation.

As regards the decided colouring of the equatorial zone, an idea suggests itself of two distinct strata of clouds, of which the lower reflect a yellower, the upper a whiter light; or the difference might have been ascribed to different thicknesses of a yellow atmosphere, but that on this supposition the tint should have grown deeper towards the limbs, which I do not think was the case. If we may conjecture such a double arrangement, it would follow that the slight variation in warmth of the planet's seasons is not very directly concerned in the distribution of these clouds, as the north or summer hemisphere possessed at once the whitest of the luminous zones, and in its grey clearings showed a yellower tinge; to which may be added what the observations appear to prove, that the coloured zone maintained an equatorial position, notwithstanding the departure of the sun from its zenith. We may, however, suppose, from the comparative transparency of the polar skies, that solar heat in general is concerned in the production of these clouds. Sufficient attention has, perhaps, not been drawn to the perspective of these atmospheric obscurations. In any oblique presentation of the luminous zones, their thickness, which we cannot imagine to be insensible, would so come into view as optically to diminish the open intervals between them; and this diminution would be progressive with the approach to the pole; so that we should have the appearance of such a continuous brightness in the arctic regions as does not seem to have been matter of observation; and hence we may infer a comparative absence of condensed vapour in those parts of the globe.

I have not been able to corroborate the general assertion that the grey belts become much lighter towards their ends. I have repeatedly remarked that they faded but little towards the limb; and, in the fine observation of Nov. 17, I have noted that I hardly thought the difference would have struck me if I had not looked for it: the principal belts could then be

readily followed quite to the limb, as in De la Rue's magnificent engraving. Nothing was ever witnessed at all approaching to the obliteration figured in Beer and Mädler's "Beiträge;" nor did the spots disappear, as those authorities found, at distances of 52° and 54° from the central meridian, though they were much more difficult objects towards the limb. It is very conceivable that the planet's atmosphere may vary greatly in different seasons, and much must be allowed for the superior power of my instrument. The effect of perspective has, however, perhaps not been always borne sufficiently in mind. A marking whose visibility depends upon its breadth will disappear from simple foreshortening in the neighbourhood of the limb, while a belt extended in the opposite direction will experience no obliteration from that cause.

The peculiar uniformity of aspect in the central region would probably be much broken up, as we know to be the case upon the Moon, by the application of greater optical power: nevertheless, enough of regularity must remain to indicate the action of some widely extended but unknown law. The very limited access which I at present possess to the necessary materials for comparison induces me to postpone an enquiry into the occasional, if not frequent, recurrence of this remarkable phenomenon; and it may well be taken up by more competent hands. Such recurrence of configuration, however, and that not confined solely to the region of the equator, may be at once indicated as matter of the highest probability. Very marvellous arrangements evidently exist upon that planet, and forces are in operation, of the nature of which we can at present form little other idea, excepting that they must declare the power of the Great Creator upon a scale of surpassing magnitude.

MICROSCOPIC TEST OBJECTS UNDER PARALLEL LIGHT AND CORRECTED POWERS.

BY THE REV. J. B. READE, F.R.S., P.R.M.S.

[PLATE LVIII.]

INTRODUCTION.

THE characters of objects we can neither see nor touch can be revealed by the microscope only. We must have a magnifying apparatus and an illuminating apparatus. The former, the magnifying apparatus, i.e. the whole battery of powers from $\frac{1}{3}$th to $\frac{1}{50}$th of an inch, may be readily obtained from Messrs. Ross, Wray, Beck, and Powell; and these eminent English opticians have spared neither labour nor expense in working up to the most elaborate formulæ. The latter, the illuminating apparatus, has much wider scope—is untrammelled by definite rules of construction, and brought out under every variety of shape. A "condenser" is what we want, i.e. an instrument for giving a proper angle to a suitable pencil of light. But what a list we have to choose from! Wollaston, Brewster, Shadbolt, Wenham, Nobert, Amici, Gillett, Kingsley, Dujardin, Reade, *cum multis aliis*, are in the field, each one offering some peculiarity of construction or some special principle of illumination. Hence the optician himself is nonplussed here, and he can only say to his customer, in the language of the showman, "Pay your money, and take your choice."

THE "KETTLEDRUM."

In attempting to make the microscope perfect as an instrument of research a universal testimony is borne to the importance of illumination; and it is not too much to say that illumination is the soul of the *complex body*, with all its ingenious mechanism, appliances, and powers. In dealing with the problem of illumination a few years ago, I felt convinced that in all our condensers a most important desideratum was wanting. We had *fixed apertures* for all the powers and for all, even the

most difficult, tests. It is impossible, however, with such an arrangement to bring out the finest markings with ease and rapidity. We might as well attempt to feed every stomach with one mouth as to feed every power with one aperture. A $\frac{1}{4}$-inch, $\frac{1}{8}$th, and $\frac{1}{12}$th require apertures of very different lengths, and hence some easy mechanical contrivance is necessary for effecting a micrometrical adjustment of the length of the aperture, and thus securing the necessary command over the size of the illuminating pencil. The well-known flute-key adjustment in my own "kettledrum," or double-hemispherical condenser, answers this purpose perfectly. The two hemispherical lenses, used also by Webster, Davis, Highley, and other observers, are, as it were, the mere raw material; the secret of success lies in the proper management of the condensed and convergent pencils. In the diaphragm cap of the single hemisphere the apertures were always of one and the same size, and hence arose a practical difficulty in using it for delicate tests. Some little coaxing of the light by means of a bull's-eye condenser between the lamp and the kettledrum was, however, an easy method of modifying the intensity of any particular pencil; but the final *flute-key adjustment* of the little sliding diaphragms gives, with the utmost nicety, just that length of aperture which the test under examination requires. The practical value of this fine adjustment is so obvious that few condensers with similarly pierced diaphragms are without some similar apparatus for regulating the intensity of the illuminating pencil.

THE ACHROMATIC CONDENSER.

It will be admitted by all that the condensers of the present day are a very great improvement on the first and early production of Dr. Wollaston. While, as to powers, the Wollaston *doublet* and Holland's *triplet* were grand improvements on Dollond's single lenses—the best powers of my youthful days—we had long to wait before the subject of illumination was practically or theoretically investigated. Here opticians and microscopists were equally at fault, and when Dr. Wollaston recommended for an illuminating lens one of three-fourths of an inch in focal length, in which the microscopic object was placed in a vortex of foci, where the rays crossed in a thousand points both before and after they fell upon the object, he failed to realise the true method of illumination. The presence of both chromatic and spherical aberration in such a construction would be fatal to success. The condenser of Wollaston was nevertheless received as an improvement over the ordinary methods of illumination, and its chief fault was subsequently remedied by Dujardin, who contrived an instrument which he termed an

éclairage for the purpose of illuminating objects with *achromatic* light. A modification of this apparatus is now supplied with all the best microscopes, and is known as the *achromatic condenser.*

THE DIATOM-VALVE.

The preceding observations on the two essential parts of a microscope clearly lead to the conclusion that we are now in possession of an instrument which is in every respect well calculated to exhibit all we want to know about an object we can neither see nor touch.

Among Nature's *invisibilia* let us select the *Diatom-valve.* A keen eye may indeed see here a minute atom, but no apparent outline; the elegant S-like shape of the *Pleurosigmata* cannot be made out, and a single hemisphere on the surface of a valve is absolutely invisible. How, then, with the aid of the microscope is the Diatom-valve described by the host of observers? So far from there being any uniformity of statement, we may almost say, *Quot homines, tot sententiæ.* The "Transactions of the Microscopical Society" contain a curious record of the Protean aspects described by different microscopists, and it is amusing to read of the ingenious modes of playing with the illuminating rays, so that the eye, fortified by a little previous theory, may see at will, in one and the same valve, either elevations or depressions, triangular, quadrangular or hexagonal dots, with rhomboids, pyramids, or spheres. In a valuable paper by Dr. Wallich, "On the Development and Structure of the Diatom-Valve," communicated to the Microscopical Society in March 1860, it is stated that "in *Pleurosigma formosum* there exists good evidence to prove that the interlinear spaces are occupied by elevated rhomboidal papillæ, which present facetted surfaces, whereas in *P. balticum,* instead of rhomboidal elevations, we have four-sided flattened pyramids, presenting as in the former case four sets of lines, of which those bounding the spaces, and not crossing them, are the predominant ones." Hence it is evident, that in spite of the all but perfect condition of our magnifying apparatus and the marvellous variety of our illuminating apparatus, the true structure of the Diatom-valve is the vexed question of the day.

In entering upon the subject myself, I took for granted that one statement, on which all were agreed, was true, and that on the valve of the *P. angulatum* there are sets of three lines in the direction of the sides of an equilateral triangle, and formed probably by elevated ridges. The shadows of these lines were generally obtained by the circle of light in the common "stop lens;" but in such condenser every portion of light, except that at right angles to the lines to be resolved, is injurious. I

therefore proposed to obtain the shadows of the lines, not by a circle of light, but by three separate small pencils of light of proper intensity in the kettledrum, to be placed by the revolution of the substage at right angles to the lines to be resolved; and if this were the true structure of the *P. angulatum*, the principle of illumination is correct. The result also appeared to be satisfactory, though in reality it only confirmed the universal error as to the existence of *hexagonal markings*, formed by the crossing of two equilateral triangles of these shadow lines. My first two papers on this subject and on the single and double hemispherical condenser are contained in the "Quarterly Journal of Microscopical Science," July 1861 and January 1867.

Still I had many misgivings as to the presence of hexagons, and, after further examination, I inclined rather to the supposition that the markings were circular and probably elevated. I therefore felt the force of Dr. Carpenter's dictum, that "*suspension of the judgment whenever there is room for doubt* is a lesson which the microscopist cannot too soon learn or too constantly practise." When I was thus shut up in Doubting Castle, asking myself the question, "Is it, or is it not?" I very fortunately purchased of my friend Mr. Powell, his little hemispherical condenser for showing the transverse lines of that prince of puzzlers, the *Amphipleura pellucida*. In using this lens I saw, upon reflection, that a small portion only was available. I therefore covered the whole lens with tinfoil, and then cut out opposite apertures on the plain and convex surface, so as to obtain in point of fact a small prism-illuminator. This answered the purpose well, and it immediately occurred to me that the equilateral prism itself might be substituted for the prism-slice of Powell's lens. I tried it, and with what result—may I say with what success?—was communicated to the Royal Microscopical Society, in papers published in the "Monthly Microscopical Journal" for July and August 1869. Mr. Slack has subsequently published two papers on this subject in the "Student and Intellectual Observer" deserving the careful attention of those who are engaged in these histological studies, and Mr. Highley has brought out a short tract on this new method of illumination with copious and interesting details.

THE EQUILATERAL PRISM.

I saw at once the great advantages of the equilateral prism and its single parallel pencil of light. I had no longer, as with the kettledrum, two suns in my firmament shining at right angles to each other, but one source of proper light properly placed; and therefore, instead of the false appearance of lines

and striæ, rectilineal and oblique under low powers, and of hexagons and other fancies under high powers, I saw what really does exist, viz. a series of beautiful hemispheres placed in their due order on the silicious tissue of the valve. If the little hemisphere under examination were really the size that our powers show it, it would be seen by unassisted vision, and we should smile at a supposed necessity of forming its shadow by two sources of light, just as an artist would smile if he were advised to have two windows in his studio at right angles to each other for the more artistic illumination of his sitter. The moon, as shown by the sun's illumination, is a fair illustration of diatom-illumination. Light, virtually parallel, falling obliquely on one side only of its mountains and craters produces *natural* light and shade. Any other arrangement would fail, and for this reason pencils of light placed at right angles or otherwise in the kettledrum lead to illusions. The kettledrum, however, with one aperture properly placed, is still a serviceable condenser, and brings out the hemispheres remarkably well. This condenser may be looked upon as monochromatic, since any one of the divisional colours of the spectrum may be used *ad libitum*. Mr. Slack very naturally prefers Ross's four-tenths achromatic condenser with a single radial aperture. Still refracted light has not the power and purity of reflected light, and converging rays refracted through a lens must yield the palm to parallel light which is obtained at the angle of total reflection in Newton's plane prism as from the sun.

Such, then, is the principle of illumination, and such the pencil of light appropriate to diatom work; and in their application I have brought out that wondrous structure, which, without the aid of the microscope, must have remained among the *invisible things* of Him who created all things.

MAGNIFYING POWER.

In arranging my microscope for work, using a Ross's $\frac{1}{12}$th and a double D achromatic eye-piece, I pull out the eye-tube until the diameter of the field of view covers exactly the magnified image of $\frac{1}{1000}$th of an inch. This diameter measured on a foot-rule is exactly 12 inches at the distance of the stage from the eye. The magnifying power is, therefore, 12,000 linear, because $\frac{1}{1000}$th of an inch on the micrometer slide lying on the stage covers 12 inches on the foot-rule. These few particulars will remove any misapprehension as to the extent of my magnifying power.

DESCRIPTION OF DIATOMS.

I will now give a more detailed account of various species of the *Naviculæ*, noticing particularly the great difference in the

relative diameters of the hemispheres, in their distance from each other, and in their angular distribution.

PLEUROSIGMA QUADRATUM.

Under my power of 12,000 linear *Pleurosigma quadratum* has 40 hemispheres and 40 intervals in the 12-inch diameter of the field, and, as each interval is equal to a radius of a hemisphere, the magnified diameter of each hemisphere covers $\frac{2}{10}$ths and the interval $\frac{1}{10}$th of an inch. Therefore, the real diameter of the hemispheres is $\frac{1}{60000}$th, and of each interval $\frac{1}{120000}$th of an inch. The rows of hemispheres cross each other at an angle of 60°, as in *P. angulatum*, and are therefore arranged in the order of the sides of an equilateral triangle. Under common methods of illumination, these markings are described and figured as hexagons, and such indeed I have often seen them myself, but the illusion arises from causing either the illuminated or the shaded portions of the hemispheres to run into each other, and so to form apparent hexagons with either dark or light centres.

PLEUROSIGMA ANGULATUM.

In *Pleurosigma angulatum* there are 48 hemispheres and 48 intervals in the 12-inch diameter of the field. The diameter of the hemispheres is less than in *quadratum*, and the interval is less than a radius. On examination it appears that one hemisphere and interval cover $\frac{1}{4}$th, or $\frac{25}{100}$ths of an inch. The diameter of a hemisphere covers $\frac{19}{100}$ths of an inch, and the width of an interval is $\frac{6}{100}$ths of an inch. Therefore the diameter is $\frac{1}{68421}$th of an inch (leaving only 1 after the fifth figure of the quotient for a remainder) instead of $\frac{1}{60000}$th as in *quadratum*, and the width of an interval is $\frac{1}{200000}$th of an inch instead of $\frac{1}{120000}$th as in *quadratum*. Larger hemispheres on each side of the strong median division add to the elegance of this favourite test, and, with the concentrated light of Brewster's hemispherical prism, the beam being rendered parallel by means of a bull's-eye lens, the *light grey* hemispheres of the valve, set off by the delicate *pink-coloured* silicious tissue, form a very charming object.

NAVICULA RHOMBOIDES.

In the *Navicula rhomboides*, a valve of exquisite beauty under prism illumination, there is an elegant border of three rows of beads or hemispheres gradually decreasing in size; then, parallel to the median line of the valve, 14 rows of much smaller beads, numbering at least 80 in $\frac{1}{1000}$th of an inch, and then

the two "median lines" which consist of hemispheres as large as those in the outer row of the border. In the centre of the valve the bos or *umbilicus* pushes out the adjacent beads of the median rows into an oval form. Curiously enough, at the preceding meeting of the Microscopical Society I had alluded to a supposed triumph of the improved kettledrum in resolving the *rhomboides* into "dots as black as jet," which, though admired by Dr. Millar and myself, are now proved, under prism illumination, to be an unnatural arrangement of light and shade.

PLEUROSIGMA FORMOSUM AND PLEUROSIGMA STRIGILIS.

In *Pleurosigma formosum* the rows of silicious hemispheres are at right angles to each other and meet the longitudinal division of the valve at an angle of 45°. In one direction there are 24 hemispheres and intervals in the 12-inch diameter of the field, i.e. in $\frac{1}{1000}$th of an inch, and in the direction at right angles to it there are 30 hemispheres and intervals, so that the rows of equal hemispheres are rather closer together in one direction than in the other. Here under the magnifying power of 12,000 linear one hemisphere and interval occupy half an inch, the apparent diameter of the hemisphere being $\frac{3}{10}$ths and of the interval $\frac{2}{10}$ths of an inch; this, of course, makes the real diameter of the hemisphere $\frac{1}{4000}$th of an inch, and therefore, with *P. strigilis*, among the largest in the range of valvular structure. We are told popularly that the "*striæ*" of diatoms range between about 30 and 100 in $\frac{1}{1000}$th of an inch, and, to adduce an example, that the striæ of *P. strigilis* are much closer than those of *P. formosum*—a statement which gives no idea of the fact that the diameters of their hemispheres are the same. Doubtless it would be more accurate to substitute the terms *hemispheres* and *intervals* for "striæ and lines," and to give the number of hemispheres in $\frac{1}{1000}$th of an inch and the measure of the space between them, or the ratio of the diameter to the interval. So well does *P. formosum* tell its own tale and set at rest the question of elevations or depressions, that a young friend of mine, under fourteen years of age, exclaimed, when he saw it, that "it looked like a plate of marbles."

PINNULARIA MAJOR AND PLEUROSIGMA BALTICUM.

In *Pinnularia major* the "stout costæ" follow the law of structure and consist of very closely-packed spheres, and in *Pleurosigma balticum* the markings, regarded by Dr. Wallich as representing four-sided flattened pyramids, follow the same type, only a larger portion of the sphere crops out above the surface of the valve.

DIATOM STRUCTURE.

The typical structure of the *Naviculæ*, as made out by this method of illumination, with even low powers as well as with a $\frac{1}{12}$th, is a series of silicious hemispheres inclined at varying angles to each other and to the median division of the valve, or rather (as a section of *Pleurosigma quadratum* reveals the fact) a series of perfect spheres of silica set equatorially in the silicious tissue of the valve. Mr. Wenham informs us that he has determined the markings of some diatoms to be spherical by the examination of fractured specimens. In one of the fragments of *quadratum* a line of spherules was detached like a row of beads, and single spherules were sufficiently detached to be examined in an isolated state under a $\frac{1}{50}$th of his own workmanship. Mr. Wenham therefore stated that I had confirmed his views in a remarkable manner.

POLYCYSTINÆ, ETC.

It is impossible to avoid noticing the stereoscopic effect of the parallel reflected light of the prism. On a Barbadoes slide of *Polycystinæ*, for instance, the objects are seen, under an inch power and on a dark ground, in very striking relief, their varied forms and characteristics being well exhibited; and the same effect is visible when viewing the proboscis of the blow-fly on a light ground. The peculiar character of muscular fibre is also well displayed, new beauty is seen in the *Podura* scale, and infusoria and portions of insects may be examined with additional interest.

TRICERATIUM AND ISTHMIA.

It seems to be owing to this stereoscopic effect of parallel light and natural shadows, that the hemispheres of diatom-valves are seen beyond all doubt as *elevations*. We seem to be looking at an opaque body illuminated from above, and the appearance in the microscope is exactly similar to a model, made to scale, in plaster of Paris. On the other hand, when we have anything approaching to *depressions*, as in the markings of *Triceratium* and *Isthmia*, these depressions are, as it were, palpably felt. The true hexagonal markings in *Triceratium* are of special interest. At every angle of the hexagon there is a hemisphere of larger size, and smaller hemispheres, in contact with each other, form the sides, so that it is questionable whether the depression is deeper than the radius of the hemispheres themselves. A similar condition also presents itself when viewing the irregular though somewhat circular markings formed by an arrangement of small hemispheres on the surface of *Isthmia*.

Thus satisfactorily are some of the minute wonders of creation, the objects we can neither see nor touch, interpreted by the equilateral prism. In its extended application it has made the microscope, old observer as I am, quite a new instrument to me, and my friend, Mr. Sheppard of Canterbury, felt constrained to say, after witnessing prism work, that "the microscope makes a new start on the Queen's birthday 1869."

THE PODURA.

Another object, which Burns would class among our "bonnie wee things," is the scale of the *Podura*, now offered to us under the more sonorous title of *Lepidocyrtus curvicollis*. In the postscript to my paper on the Equilateral Prism I offered a contribution on its structure, and attempted to remove some prevailing misapprehensions. This minute scale, a very favourite test with practical opticians, appears under prism illumination to consist of two membranes, between which there is a series of small solid spherules or beads. Under a power of 12,000 linear, I find 24 beads in $\frac{1}{1000}$th of an inch on the 12-inch horizontal diameter of the field, and 6 on the vertical. Hence, in the latter direction they are about $\frac{1}{6000}$th of an inch apart, and in the former, the interval being equal to a diameter of a spherule, they are about $\frac{1}{48000}$th of an inch apart. This is the utmost I have been able to accomplish. That I have not done more is an indication either of want of skill in the observer, or of want of penetration in the magnifying power, and fortunately recent examination has proved that neither the observer nor his illuminating apparatus is in fault. The defect is in the power.

DR. ROYSTON PIGOTT'S RESEARCHES.

When my own $\frac{1}{12}$th, a choice power, carefully selected for me by Mr. Ross, or an almost perfect $\frac{1}{5}$th by Wray, is removed from my own microscope to Dr. Pigott's, it is brought under the influence of his "Aplanatic Searcher," a system of lenses, between the eye-piece and the power, which detects and remedies residuary spherical aberrations, and I can now see with my own powers what had been heretofore invisible, viz. the beautifully beaded structure of the whole test scale as discovered and described by Dr. Pigott. Mr. Slack, himself an eye-witness, thus announces the interesting discovery:—

"Dr. G. W. Royston Pigott has recently astonished the world of microscopical observers, by telling them very plainly two startling, and to many unwelcome, *truths*, for such we must pronounce them. First, he says that they have not seen their favourite test object, the *Podura* scale (*Lepidocyrtus curvicollis*),

properly; secondly, that their best object-glasses are afflicted with sufficient spherical aberration to have rendered the structure which he describes invisible."—"The Student," Feb. 1870.

Dr. Piggott's views "On High Power Definition" were communicated to the Royal Microscopical Society at the end of May last, and his paper, read after the summer vacation, was published in the December number of the "Monthly Microscopical Journal."

Mr. Slack states that "all *difficult seeing* is in some suspense through Dr. Piggott's researches;" and it really seems to me that microscopical results are now in a transition state. Two well-known facts point to this conclusion.

First, the extraordinary advantage of employing either parallel rays, or rays of small convergence in illumination.

Second, the better correction of spherical aberration.

Dr. Piggott, like myself, generally employs a single illuminating pencil, in his case virtually parallel, and therefore I am not surprised at his announcement of "Crescentic shadows" as proving that elevation of diatom-markings which I had detected myself not a week before his paper was written. There are, indeed, many facts and secrets of structure which can be revealed by such a beam alone. Light virtually parallel can lead to no confusion, but the crossing and recrossing of an infinite number of rays produce such a multiform shadow as to de-shadow or obliterate a true light and shade portraiture, which is the essence of every picture, and the very soul of every natural representation. Happily there is no difficulty in obtaining parallel rays of sufficient intrinsic brightness. If in any case the parallel simple beam from the equilateral prism may be found, as it were, too diluted, the parallel condensed beam from the hemispherical prism will be a ready and sufficient substitute.

Dr. Piggott has also made a very decided advance in the better correction of residuary aberration, a point which has, I believe, been almost completely ignored—nay, even denied—until recently, by accurate observers as well as by distinguished opticians. From my own experience in Dr. Piggott's studio, I have no doubt that his *colour test*, a most interesting feature in his experiments, is the result of his finer balance of the aberrations. Before I had the pleasure of seeing Dr. Piggott, I had an intimation of the "colour test," as noticed in my description of *P. angulatum*, but only to the point at which residuary aberration stopped it. For the very same power, under the influence of Dr. Piggott's correcting lenses, shows intensity of colour, and the beads of *Degeeria plumbea* appear a brilliant azure blue in the deeper focal plane, and of a rosy radiant pink in the upper plane. This new fact is perhaps one of the most

striking phenomena in microscopical science of the present day.

Whether this colour test, for which we are indebted to Dr. Piggott, is explained on the theory of the vibrating wave lengths corresponding to the infinitesimal thickness of films, as in the gorgeous colouring of the soap bubble, or is dependent upon a peculiar compounding of the colours by the glasses themselves, or upon the irradiation, refraction, and internal reflections of the spherical beads of which all scales and diatoms appear to be built up, are questions of so recondite a nature as to be worthy the consideration of the most advanced physicists of the day, and I hope Dr. Piggott will not be left alone in his glory.

POLARISED LIGHT.

Returning for a moment to the equilateral prism, I will conclude my suggestions for examining objects we can neither see nor touch, by recording the fact that the prism is an admirable *polariser*. While the light obtained at the angle of total reflection from an inner face of the prism is not perceptibly altered either in nature or quantity, yet the same incident pencil, if reflected from an outer face of the prism, supplies a *purely polarised beam*. As such, I have used it in the examination of several suitable objects, passing it through selenite plates where necessary, and I prefer the results to any previously obtained by the direct light of a common Nicol's prism. Salicine and crystals generally, as well as fine vessels in animal and vegetable tissues, are seen in almost stereoscopic relief, in consequence of the shadows which are readily thrown by a slight obliquity of the polarising pencil. This obliquity may be extended to the bringing out the effect of polarised light, even on a dark ground, and thus, as in the combination devised by Mr. Furze, heightening the solidity by the play of colours.

The prism when adapted to the microscope is mounted on a small arm with ball-and-socket joint, and, by varied adjustments of the prism, direct, oblique, dark-ground, and polarising illumination may be produced; so that an object may in turn be investigated by every kind of illumination without change of apparatus or readjustment of the object. Mr. Highley, at my request, has brought out a *popular instrument*, in which the expensive luxuries of a mechanical sub-stage and elaborate condensers are dispensed with, and his *prism microscope*, consisting of the body with its powers, a thin stage, and a two-inch equilateral prism looks like a good working tool, and cannot fail to interpret the minute wonders of creation to the intelligent admirer of nature.

The following explanation is from the pen of Dr. Pigott, who kindly supplied the illustrations for this paper.

EXPLANATION OF PLATE LVIII.

BY DR. ROYSTON-PIGOTT, M.A., F.R.A.S., F.R.M.S.

LATE FELLOW OF ST. PETER'S COLLEGE, CAMBRIDGE.

THE accompanying diagrams have been delineated under the magnifying power of a half-inch and one-sixteenth Powell and Lealand immersion lens,* the only kind of objective which has resolved Nobert's nineteenth band of lines drawn at about 112,000 per inch. The power varies from 500 to 5,000 diameters. Most of the appearances have been exhibited to Mr. Reade, at whose request the drawings have been made, who will readily recall one of the novel *Podura* appearances, No. 4, a valuable preliminary definition, as one among the manifold changes which this precious object assumes, according to the light, quality, and corrections of the microscope. The double black interference lines on the provisional ribbing, as represented by fig. 9, is another astonishing effect, occasionally also to be seen with the striated beading shown at figs. 7, 8; whilst fig. 6 is a particularly striking result of purposely changing the spherical aberration of the objective. Fig. 7 represents the lower stratum of beading seen with the immersion one-sixteenth and oblique light. The upper rows are only to be seen as aided by a very minute brilliant achromatic pencil of illuminating rays: 8 gives both beadings at once. Fig. 5 represents the false standard appearance of the *Podura* test, accredited for many years by microscopists.

Diatoms.—Fig. 1 is a faint resemblance to the form of the resplendent beading of the highly refracting silicious substance of the *Pleurosigma formosum*, which ranges about 32,000 to the inch, with an immersion power of 5,000 diameters. Crescent shadows are sharply displayed, and the beading, glowing with a deep ruby tint, is crowned with tips of golden light. In the interstitial spaces fine traces of a brilliant blue colour, lost in the azure brightness, may be observed of a secondary structure, resembling spherules. But at present they appear too spectral to be definitely discussed; but they form a most vivid contrast to their ruby companions in the field. It would seem very little light is lost by transmission. In proportion as the definition becomes finer the light appears to increase, even with the same source of light and reduced pencil.

Large isolated beads, scattered by twos and threes out of the

* Nearly one-twentieth of an inch focal length, with the adapted water lens.

usual rowlets, are very remarkable, and are found along the median and boundary lines, and in the open beading form clusters of dark irregular spots. The bead-shadows or crescents can be made to keep their original azimuth by revolving the stage; proving their sphericity. I have only recently discovered that the median line of the *formosum* is composed of the four rows of minute beads shown in the figure.

Fig. 2.—*P. angulatum*, nearly three times smaller than the *formosum* in its beading, presents similar characters in the main.

Fig. 3.—The *P. rhomboides* can be beautifully shown with the immersion one-sixteenth, and probably equals the Nobert-line test in its exquisite dotted system, because a line is more easily seen than dots; and a line of dots is more visible at a given distance in proportion as the dots are placed closer together.

That the diatom spherules are veritable lenses of a convex form is evident from their appearance under the finest microscopes and from their optical properties, which justify the conjecture that at no distant date they will be used to form focal images for the purpose of perfecting objectives themselves, when the greatest advance will have been made that opticians have ever accomplished in microscopical coustruction.

WHAT IS WINE

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HAVING in two former communications confined our attention to the subject of wine in general, we may now proceed to examine some of its many varieties, employing the knowledge previously gained to obtain some insight into their more prominent chemical characteristics.

We will select for the purpose the principal varieties imported into England, and as one wine must necessarily be placed first, Rhine wine may perhaps most fitly occupy this post of honour, since its finest growths are almost universally admitted to be of unrivalled excellence.

Rhine wine (Rhenish wine).—This name was originally confined to wines grown in the Rhine-gau, but as several of the wines produced in surrounding districts were found to possess a very similar character, they have been included in the above general term. Such is, for example, the case with the wines grown near the town of Hochheim, lying a little to the south of the Rhine-gau on the right bank of the Main, which rank among the best Rhine wines, and have, indeed, supplied us with the generic term of Hock by which we designate all better sorts of Rhine wine.

The Rhine-gau stretches along the right bank of the Rhine from Biberich to a little below Rüdesheim. It is about twelve miles long by six miles broad, and its inhabitants are almost exclusively engaged in the cultivation of the vine. The average annual production of the Rhine-gau, including, however, Hochheim, may be taken at about 3,000,000 gallons. The quantity produced on the whole German part of the river rises to something like 26,000,000 gallons.

All Rhine wines are white, or rather pale yellow, with, perhaps, the single important exception of that grown near Asmannshausen, which is red. The most celebrated are those of Schloss Johannisberg, Steinberg, Märcobrun, Rüdesheimer

Berg, and Hinterhäuser, Geisenheimer, Rothenberg and Hochheimer, Dom-Dechanez. The above localities supply, nearly every year, wines of first-rate quality, and in good seasons furnish, beyond comparison, the finest white wine the world produces.

Rhine wine is characterised by a moderate alcoholic strength, from 7 to 12%, a moderate amount of acidity, chiefly fixed acids, among which tartaric acid is probably never absent. It contains a high proportion of volatile ethers relative to its alcohol, and a very small proportion of ash. In this ash carbonates and chlorides are scarcely ever absent, and there is but little sulphate of potassium. Albuminoid substances and sugar are never present in quantity, having both been almost completely removed in the course of a very perfect fermentation. The wine is pure and therefore wholesome, has a very characteristic, and often an exquisite, bouquet, and is of extraordinary keeping quality, wines 200 years old and upwards being still extant and in good condition.

Claret.—Claret is a red wine produced in the district of the Médoc and other parts of the Département de la Gironde. The district of the Médoc, where the finest wines are grown, extents a little northward of Bordeaux, along the left bank of the Gironde. It contains about 40,000 acres of vineyards and produces annually nearly 8,000,000 gallons. The lighter clarets are grown on the right bank of the river. The wines of the above districts include the finest growths of France, the most celebrated being Châteaux Margaux, Châteaux Lafitte, Châteaux Latour, and Châteaux Haut-Brion.

Claret is also generally a thoroughly fermented wine of moderate alcoholic strength (8 to 13%) and acidity, the latter, however, being due in greater proportion than in Rhine wine to acetic acid. This is indeed the case, as before explained, in red wines generally. Among the fixed acids tartaric acid is rarely or never absent. It contains little or no sugar, a high proportion of volatile ethers, much colouring matter, tannin, and some albuminoid substance, this being, however, preserved from change by the tannin present. The wine leaves but little ash, and this of much the same composition as in Rhine wines.

As a general beverage, more particularly when diluted with about half water, the lighter sorts of claret are perhaps the most wholesome of all the different wines imported; while the finer growths, in regard to body, flavour, and bouquet, are unquestionably the finest examples of red natural wines.

Hungarian Wines.—Hungary is one of the chief wine-producing countries of Europe, the vine being cultivated almost throughout the whole country, and yielding annually nearly

400,000,000 gallons of wine. These wines are both red and white, and the Hungarians enumerate many hundred varieties, most of which show, however, great general similarity. The most celebrated is that known as Imperial Tokay, grown on an estate belonging to the Emperor of Austria, and it ranks among the choicest wines of Europe.

Hungarian wines are, with few exceptions, like the preceding, thoroughly fermented, and contain therefore little or no sugar and albuminoid substance: their alcoholic strength ranges from 7 to 12%. They are generally rather more acid than either Rhine wine or claret, owing chiefly to a greater proportion of volatile acid, due probably to a less perfect treatment of the *must* or negligence in the storing of the new wine. They leave but little ash, which is of the usual normal composition and does not, as is sometimes asserted, contain more phosphoric acid than other natural wines. Like all unbrandied wines, they contain more volatile than fixed ethers. At present, the wines of Hungary are on the whole inferior to both Rhine wine and claret. The country possesses, however, a climate and soil apparently excellently adapted to the cultivation of the vine, and if the growers will take a lesson in the art of wine-making from either France or Germany, they may in time rival both in the quality of their produce, while even now they are very far ahead of the latter, as far as quantity is concerned.

The three wine countries just considered all enjoy a temperate climate; in general, warm enough in summer to bring the grape to maturity without overloading it with sugar, cool enough in autumn to allow the *must* to be thoroughly fermented without danger to the wine. They are emphatically the three natural wine-producing countries of Europe.

Greek Wines.—Greece produces a considerable quantity as well as a great many varieties of wine, both on the main land and on the surrounding islands. Judging, however, from its geographical position and general climate, it would appear to be but ill adapted for the production of a sound and perfectly natural wine, and this conclusion is fully borne out by the chemical character of many of the wines produced.

A great variety of Greek wines are imported into this country, many of them slightly fortified, the alcoholic strength ranging from 8 to 14%. They are generally characterised by a rather high percentage of acid, due chiefly to an unusually great proportion of acetic acid, which indeed in some of these wines is present in excessive quantity, equalling or even surpassing the fixed acids in amount. Besides this great proportion of acetic acid, Greek wines frequently contain an appreciable amount of aldehyde, a product intermediate between alcohol and acetic

acid, being in fact the first stage in the conversion of alcohol into vinegar.*

All this proves that Greek wines are either extremely liable to a slight acetous fermentation, which, as they often contain much albuminoid substance, is not unlikely, or that owing to bad management the *must* or new wine is improperly exposed to the ordinary action of the air. The wines leave a rather high proportion of ash, generally rich in sulphates, which, taken in conjunction with the small amount of tartaric acid they contain, indicates that most likely plaster of Paris is used in their preparation. Lastly, Greek wines, in spite of the high proportion of volatile acid present, frequently contain more fixed than volatile ethers, contrary to what was found to be the case with pure natural wines, and resembling in this respect fortified wines generally.

Taken as natural wines they will bear no comparison with the three previously considered, either as regards soundness, and consequent wholesomeness, or in regard to the perfection of their flavour and bouquet; whilst as strong wines they are greatly inferior to those next to be considered.

Sherry.—This name (corrupted from Xeres) was originally restricted to the wine produced in a district of somewhat triangular shape (each side of the triangle being about 12 miles long), at one corner of which the town Xeres de la Frontera is situated. The use of the term has, however, been gradually more and more extended, until at last nearly every white Spanish wine is included in it. The town of Xeres forms the chief depôt for sherry, and vast quantities are stored there in warehouses called *bodegas*. In Spain, unlike to what is the custom in France or Germany, the wine is but very rarely named according to the town or district where it is grown; indeed, it would seem that the system under which the wine trade of Xeres is carried on renders such a method of classification impossible. The wine is, however, frequently distinguished according to the particular flavour it possesses, as Amontillado, for example. Spain produces about 140,000,000 gallons of wine yearly, of which about 4,000,000 are the produce of the Xeres district.

All sherries, even those imported as natural sherry, are fortified, and their alcoholic strength depends, therefore, upon the amount of spirit added; it varies usually between 15 and 22%. The amount of acid present is, generally speaking, less than in the preceding wines, though not nearly to the extent their taste

* Among a very great number of wines analysed, other than Greek, I have found aldehyde in only two samples, one a Sautern the other a Rhine wine. In both instances the bottles of wine had purposely been placed, for nearly a year, upright and badly corked, in an ordinary sitting-room.

might lead us to suppose, being more masked by the spirit, &c. The proportion of volatile acid is about the same as in claret, and they contain little or no tartaric acid. The amount of sugar is also very variable, ranging from 0 to 4% and more. Of all the fortified wines examined, sherries are the only ones, Madeira excepted, which, as a class, contain more volatile than fixed ethers. All true sherries leave a high proportion of ash, owing to the universal practice of adding plaster of Paris to the *must*. This ash consists chiefly of sulphate of potassium,* and contains frequently neither carbonates or chlorides, though the latter are never absent from the wine itself. Phosphates are also present in small proportions only, most likely also owing to the practice of plastering, by which some of the phosphoric acid of the *must* is precipitated.

Sherry, as indeed all fortified wines, can scarcely be regarded as a beverage, except perhaps when diluted with water. As a stimulant it is, however, of considerable virtue, and though it is frequently asserted that its action is due to the high percentage of alcohol, it is in many cases far preferable to brandy. As wines to be sipped, not drunk, the better sherries form an extremely fine class, possessing strength, sufficient body, fine flavour, and very exquisite bouquet, but to attain this perfection they must be kept for many years in bottle.

Madeira.—This wine, as its name implies, comes from the island of Madeira, which, though not large, produced formerly upwards of 2,000,000 gallons yearly. Owing to the prevalence, during several years, of the oïdium, or grape disease, which destroyed the vines, the yield was reduced to next to nothing, and many vineyards were uprooted and used for the cultivation of sugar-cane. At present, however, the vines have either been replaced or are in course of replacement in most of the old vineyards, and the production of the island has again risen to above 500,000 gallons annually.

Madeira, like all southern wines, is fortified, its strength varying usually between 17 and 20%. In its general characters it greatly resembles sherry: like this, it has a greater proportion of volatile than fixed ethers, contains about the same amount of acids and sugar, and is also almost free from tartaric acid. It leaves rather a high proportion of ash in which sulphates pre-

* Many sherries contain as much as sixty grains of sulphate of potassium per bottle; in other words, a strong purgative dose even for an adult. Potassium salts, when given in large doses, have moreover a depressing effect on the heart's action, and it is well worthy of careful observation whether the good effects intended to be produced by the administration of the sherry, is not sometimes completely counteracted by the large dose of sulphate of potassium given simultaneously. In such cases Madeira might perhaps be substituted for the sherry with advantage.

dominate, though to a less extent than in sherry. Indeed, the art of cultivation of the vine and the treatment of the *must*, &c., is nearly the same as in Spain. East and West India Madeira is such as has been shipped once or twice to one or other of the above places respectively, or has even been stored there for some time. The high temperature to which the wine is thereby exposed considerably facilitates the maturing of the wine, which thus improves more rapidly, and perhaps to a higher degree, than it otherwise would do. Madeira is a full-bodied wine, of excellent keeping quality, and when mellowed by age or high temperature, of extremely fine flavour and aroma.

Port Wine is the produce of a district called the Alto Douro, or Upper Douro, about 50 miles east of Oporto in Portugal. The vineyards are situated on both banks of the river, and the wine produced is sent by boats to Oporto, where it is classified, stored, &c. From the circumstance of its being exported thence is derived its generic name of port. The district of the Alto Douro is capable of producing, in favourable seasons, upwards of 8,000,000 gallons of wine. Port wine is also a brandied wine, its alcoholic strength ranging generally from 15 to 20%. The total amount of acid it contains is somewhat less than in Rhine wines, but the volatile acid is somewhat greater in amount, and it contains scarcely any tartaric acid, cream of tartar being almost insoluble in the strong wine. In spite, however, of this higher proportion of volatile acid, the volatile ethers are found in less proportion than the fixed ethers; the wine, when young, has in consequence less bouquet than the preceding. As fermentation has been prematurely suppressed the wine contains a variable amount of sugar, depending of course upon the period at which fermentation was interrupted, or on the amount of inspissated *must* added subsequently; it averages from 2 to 6%. It contains also much tannin and colouring matter from the stones and husks of the grape (or elderberry?), and comparatively a large proportion of albuminoid substance which, owing to incomplete fermentation, has not been precipitated. It leaves in general but a moderate amount of ash, though rather more than either hock or claret, and of very much the same composition as in these two wines.

As the wine gets older, some tartar, much colouring matter, tannin, and albuminoid substance are thrown down, and it loses some of its sugar and increases in acidity (acetic acid). Meanwhile, the volatile ethers appear gradually to gain on the fixed ethers, so that in old wines they equal or even exceed them in amount; the wine approaches in this respect more and more to the natural standard.

Port wine, when new, has but little bouquet, is rather rough

and astringent, and, on account of the large amount of spirit added, has rather a burning taste. It is at the same time very fruity, owing to the large quantity of unfermented grape-juice it contains. However, when mellowed by age, it has lost these disagreeable qualities, retains only a moderate fruitiness, has an excellent flavour and bouquet, and is then undoubtedly a very superior generous drink, which, if taken in moderation, is both exhilarating and strengthening.

Marsala is a Sicilian wine, produced in the district of Marsala, at the western extremity of the island. This district yields about 3,000,000 gallons yearly, the produce of the whole island averaging 8,000,000 gallons. Most of the Sicilian wine is shipped from the port of Marsala, and no doubt much of what is exported as Marsala comes from various other parts of the island.

Marsala is a wine fortified by spirit, and generally sweetened by the addition of sugar. The wine is usually well fermented, in consequence of which it contains only traces of albuminoid substance, and is therefore of good keeping quality. It has a very small amount of acid, being, in fact, one of the least acid of wines. It is thus strong, sweet, and but slightly acid—to which combination, no doubt, being also otherwise a sound wine, it owes its great popularity. The wine leaves a somewhat high proportion of ash, though not so high as sherry, and in this ash sulphates are rather prominent, suggesting the use of plaster of Paris; as in most fortified wines, fixed ethers are present in higher proportion than volatile ethers. This fact constitutes a striking difference between Marsala and sherry, the lower qualities of which it otherwise resembles.

In conclusion, I append a few analyses of each class of wine described. Those of my readers who have thus far followed me will understand that, inasmuch as in the same class of wine every constituent varies between wide limits—nay, as even one and the same wine varies from year to year—it is impossible to give an analysis of one or more wines to represent a class; accordingly, every analysis in the table must be taken as representing only the one particular sample of wine analysed, and should on no account be regarded as a mean, or a representative, of its whole class. An attentive examination of the table will, nevertheless, enable anyone to trace some of the more striking characteristics of the several classes alluded to in the preceding descriptions. See, for example, columns 4 and 5 showing the amounts of volatile and fixed acid; or columns 10 and 12, giving the proportions of ash, and sulphates, and chlorides respectively; or, finally, the last four columns, relating to compound ethers. The table, moreover, will, I believe, be found to give the most complete analyses of the several wines

WEIGHT IN GRAMMES OF SOME OF THE CHIEF CONSTITUENTS OF 1 LITRE (1000″ OR 1000 VOLUMES) OF THE UNDERMENTIONED WINES.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Particulars of wines analysed	Specific gravity	Absolute alcohol	Free fixed acid in tartaric acid	Free volatile acid in acetic acid	Total free acid in tartaric acid	Real tartaric acid	Total dry residue	Grape and fruit sugar	Total amount of ash	Carbonate of potassium	Sulphates and chlorides	Phosphate and carbonate of calcium	Total amount of phosphoric acid	Alcohol in fixed ethers	Alcohol in volatile ethers	Total alcohol in ethers found	Total alcohol in ethers calculated	Proportion per cent. of alcohol found to alcohol calculated
Hock, white 30s. per doz. vintage 1862	993·43	95·6	3·48	0·57	4·20	—	18·63	—	1·95	0·58	0·76	0·60	0·32	0·132	0·230	0·362	0·360	100·5
Hock ,, 40s. ,, ,, 1859	993·48	92·0	4·20	1·14	5·62	2·550	18·55	0·12	1·70	0·07	0·78	0·85	0·30	0·199	0·239	0·438	0·458	95·7
*Hock ,, 120s. ,, ,, 1857	992·81	104·4	4·31	0·93	5·37	0·675	20·60	1·12	1·45	0·14	0·46	0·85	0·35	0·225	0·239	0·464	0·493	94·3
Claret 15s. ,, ,, 1865	995·58	85·3	4·24	1·47	6·08	0·675	21·40	4·31	2·08	0·66	0·95	0·48	0·35	0·155	0·197	0·352	0·476	74·0
Claret 48s. ,, ,, 1865	995·03	120·0	4·24	1·74	6·41	1·875	24·33	2·04	2·25	0·66	1·05	0·55	0·30	0·186	0·248	0·430	0·581	74·6
Claret 66s. ,, ,, 1861	994·73	85·3	3·23	1·80	5·48	1·838	18·00	0·95	2·00	0·38	0·99	0·63	0·30	0·166	0·216	0·382	0·429	88·8
Hungarian, red 21s. ,, ...	992·07	113·6	3·56	2·49	6·68	0·600	20·85	1·47	1·85	0·41	0·91	0·53	0·35	0·151	0·358	0·509	0·656	77·6
Hungarian, white 34s. ,, ...	992·88	95·4	5·33	1·47	7·16	0·675	18·20	0·61	1·75	0·14	0·81	0·80	0·25	0·186	0·271	0·457	0·613	74·5
Hungarian ,, 42s. ,, ...	993·09	94·9	4·74	1·80	6·99	0·375	18·13	0·24	1·88	0·12	0·90	0·85	0·25	0·162	0·273	0·435	0·596	73·0
Greek wine, white 20s. ,, ...	994·56	107·2	3·41	3·00	7·16	0·675	25·30	2·00	2·25	0·07	1·18	1·00	0·25	0·224	0·214	0·438	0·690	63·6
Greek ,, ,, 28s. ,, ...	992·25	124·5	4·54	1·68	6·64	—	24·42	1·12	3·05	0·41	2·01	0·62	0·25	0·384	0·179	0·563	0·707	79·6
Greek ,, ,, 36s. ,, ...	993·17	138·9	2·33	1·77	4·54	0·300	25·50	3·64	3·75	0·21	2·49	1·05	0·45	0·245	1·207	0·453	0·530	85·1
Sherry 22s. ,, vintage 1865	994·09	172·0	2·70	1·53	4·61	0·187	42·00	25·65	4·50	0·07	3·63	0·80	0·18	0·206	0·216	0·422	0·639	66·1
Sherry high price ,, 1860	997·93	178·1	3·08	1·68	5·18	0·262	53·50	29·70	5·50	0·18	4·41	0·95	0·25	0·290	0·391	0·681	0·749	90·8
Sherry ,, ,, ,, 1857	998·30	184·0	2·81	1·62	4·84	0·150	56·44	35·10	5·13	0·07	4·18	0·88	0·13	0·262	0·469	0·731	0·722	101·2
Madeira, East India, 60s. per doz. . . .	993·94	177·5	3·26	1·68	5·36	0·300	43·47	20·80	3·90	0·27	2·52	1·10	0·42	0·305	0·382	0·687	0·774	88·7
Madeira high price vintage 1812	994·15	180·0	4·20	3·27	8·25	—	45·41	16·29	3·59	0·17	1·93	1·49	0·50	0·460	0·773	1·233	1·207	102·1
Port 32s. per doz. ,, 1864	1004·76	185·6	3·08	0·84	4·13	0·225	75·37	43·31	2·48	0·48	1·34	0·65	0·35	0·302	0·128	0·430	0·620	69·4
Port high price ,, 1854	997·42	175·3	3·54	1·07	4·88	0·225	53·90	22·84	2·58	0·66	1·37	0·55	0·33	0·351	0·220	0·571	0·697	84·9
Port ,, ,, ,, 1842	986·95	182·6	2·66	1·08	4·01	0·150	31·01	10·10	2·10	0·69	0·86	0·45	0·33	0·288	0·331	0·614	0·595	108·2
Marsala 16s. per doz. old	996·65	167·1	1·88	1·11	3·26	—	49·83	32·40	2·25	0·21	1·64	0·50	0·18	0·256	0·189	0·445	0·447	99·3
Marsala 20s. ,, very old	999·65	168·9	2·25	1·38	3·98	0·150	57·46	37·60	3·13	0·55	1·92	0·65	0·23	0·333	0·216	0·549	0·550	99·8

included as yet published, and will, I trust, therefore, be of value and interest to many, even quite independently of its connection with the preceding papers.

The arrangement of the table will be easily understood. All the figures, with the exception of those in columns 2 and 19, give the number of grammes of the different substances named at the heads of the columns contained in 1000″ (1 litre) wine. By shifting the decimal point one figure to the left, we therefore obtain percentages; and by multiplying the figures by 12, the number of grains of the various constituents contained in one bottle of wine are obtained. (An ordinary wine-bottle holds about 12,000 grains.) Column 2 gives the specific gravity of the wine, i.e. the weight of 1000″. In column 19 will be found the proportion per cent. which the alcohol in column 17 bears to that in column 18; in other words, it will show us at a glance how near to, or how far from, equilibrium the process of etherification was at the time of examination (in these cases, the latter half of 1867). Lastly, as regards column 1, I have contented myself with simply giving the generic name of the wine, together, when possible, with the year of vintage, and the retail price per dozen in London. Everyone may thus, to a great extent, judge of the quality of the wine analysed by the price appended. The wines were selected with great care, to ensure, as far as possible, their genuineness; but, for obvious reasons, the sources whence they were derived are withheld.

THE FERTILISATION OF VARIOUS FLOWERS BY INSECTS. (COMPOSITÆ, ERICACEÆ, &c.)

BY WILLIAM OGLE, M.D., F.R.C.P.

[PLATE LIX.]

A VERY short time ago it was considered a sufficient explanation of the various colours and forms of flowers to say that they were devised to please the eye of man by their brilliancy and their variety. This no longer satisfies us. For we have learnt from Mr. Darwin, that every detail of structure in an organism exists purely and solely for the sake of that organism itself, or because it was of use to the ancestors of the organism, and has been derived from them by inheritance. If anyone bear this general law in mind, and examine by its light a number of different flowers, he will, I think, be led inevitably to two conclusions. Firstly, he will be convinced that the purport of a nectary is to attract insects, and that those flowers which possess one require the visits of insects for their due fertilisation. A second conclusion will be this: That any notable irregularity of the corolla is also—if not invariably, yet usually—connected with the visits of insects, and has, like the nectary, for its ultimate object fertilisation by their agency. The manner in which the irregularity acts is not always the same. One very common result of it is to compel the insect to visit the nectary in some particular direction by barring up all others, that particular direction being such that the insect is made to impinge in a useful way upon stigma or upon anthers, as the case may be. Another very frequent purport of the irregularity is to compel the insect to alight on a particular part, where its weight causes certain mechanical effects by which the pollen is transferred to the body of the insect and is then carried off to the stigma, perhaps sometimes of the same, but more frequently of some other, blossom.

This view of the purport of irregularity throws light on a fact noticed in manuals of botany, but hitherto, so far as I know, un-

explained, viz. the manifest connection which exists between the presence of nectaries and irregularity of the corolla. "It is to be remarked,"* says A. de Jussieu, "that the development of a nectary on any particular part, stands frequently in intimate relationship with the irregularity of the flower, and seems to determine irregularity on that side where it is situated." This intimate connection of two structural peculiarities becomes perfectly intelligible if it be admitted, as urged above, that the two have one common object, viz. the promotion and utilisation of the visits of insects.

I have already in two former numbers of this Review† given sundry illustrations of the preceding remarks. To those illustrations I would now add some others. Combining these with the former, I shall, I think, have shown reasons for concluding that nectaries and irregularity have for their final cause the promotion of intercrossing, and that the same purpose is often subserved by other structural peculiarities which might at first seem matters of indifference. Such, for instance, are the coherence of the anthers (e.g. *Gesenria*, *Composita*); their irregularity in form (e.g. *Erica*, *Salvia*), in number, length, or general arrangement (*Didynamia*); their mode of dehiscence (e.g. *Vaccinium*, *Arbutus*, &c.), its period (e.g. *Digitalis*), and the direction of their dehiscent surfaces (e.g. Thyme, &c.). Such also are the set of the flowers on the stem (e.g. *Melampyrum* and *Pedicularis*); their mode of inflorescence (capital); the distribution of their colouring (*Pelargonium*), and perhaps even the size of the calyx (*Pedicularis*).

I have already pointed out‡ how in Thyme and Marjoram intercrossing is much facilitated by the close crowding together of the flowers, in some of which the stigmas, in others the anthers, are alone mature: because a bee, crawling over the flowery surface, must inevitably convey pollen from blossom to blossom. The still closer crowding of the capital and the *umbel* may perhaps have a similar result. If it be so, we are furnished with the *raison d'être* of these modes of inflorescence.

As far, at any rate, as regards the capital, facts, I think, justify this supposition. If we examine a flower-head in any of the thousand composites, we find as follows: Firstly, in each individual hermaphrodite flower the pollen ripens and is exposed before the stigma is mature. Secondly, the different flowers in the same head do not expand simultaneously, but the expansion begins with those of the circumference and extends centripetally. There are thus collected together flowers in every stage. Some in the centre, not yet open: outside these a ring of others, in

* "Botanique," ninth edition, p. 312.

† See the July number of last year, and the January number of this year.

‡ P. 53.

which ripe pollen is exposed, while the stigmas are yet immature: still more externally, flowers in which the stigmas are mature and exposed, but from which the pollen has mostly or altogether vanished, having been carried off by insects. It is evident how the different periods of pollen and stigmas in each blossom hinder self-fertilisation, and how, on the other hand, the close crowding of flowers in different stages, favours intercrossing.

The common Feverfew will serve as an example. Here each disk-flower is furnished with both pistil and stamens. The five anthers are united so as to form a tube, closed at the upper end (fig. 1), and they dehisce on their inner surface, that is, inside the tube. In this latter is lodged the style, which in the yet immature flower is only so long as just to reach into the lower end of the tube with its upper extremity. This extremity is bifid, but the two segments are held in close contact with each other, there being no room in the tube for them to diverge laterally. The stigmas are bends on the borders of the inner surface of these segments, and are therefore not exposed to any great extent, so long as the two segments are in close contact. At the upper end of each segment is a tuft of hair-like papillæ, which are so set that the whole style, while it is in the tube, somewhat resembles a besom with the handle downwards and the twigs uppermost. When the flower first expands the pollen is already ripe, and lies in that part of the closed anther-tube which is above the hairy summit of the style. The style, however, continues to grow, and as it lengthens the broom-like end sweeps out the pollen cleanly from the dehiscent cells, and, forcing it upwards, makes it break open the closed end of the tube and overflow on the surface in a confused mass (fig. 2). Hence it is soon removed by the insects which crawl over the flower head, and by the time that the style itself protrudes from the tube, all or most of it is usually already gone. The bifid style now itself emerges, and its two segments soon separate from each other and expose their stigmatic surfaces (fig. 3). To these a bee can scarcely fail to convey pollen from the close adjoining flowers, which lie next towards the centre and are less mature. Doubtless, also, it will not rarely happen that some of the flower's own pollen will still remain scattered over the petals, and that some of this will be conveyed to the stigma and self-fertilisation occur. But most of this, and often all, as already mentioned, has already gone; whereas the pollen of the more central flowers is yet undiminished in amount. The flower also itself is but one, while its less mature neighbours are several, so that the chances are largely in favour of intercrossing. This, therefore, and not self-fertilisation must be the rule.

To speak of the tufts of hair-like papillæ as collecting pollen

for the fertilisation of the flower is clearly erroneous. Their position shows that their use is the very opposite. They serve to sweep the pollen out of the way, and prevent it from reaching the stigma. It would be better, then, to call them *poils expulseurs* than *poils collecteurs*, by which latter title they are known in French manuals.

As the expansion of the flowers begins at the circumference, and thence extends towards the centre, it is plain that the pollen of any given flower must be used to fertilise another placed more towards the periphery than itself. What, then, of the most peripheral of all? Their pollen can be of no use to flowers in the same capital, for none lie outside them. It is intelligible, therefore, that these flowers should cease to produce a comparatively useless product; and accordingly we find the ray-flowers destitute of stamens, though still provided with a pistil. As there are no stamens there is again no necessity for a brush to sweep out the pollen, and accordingly we find that the hair-like papillæ are quite rudimentary in the style. It is the saving thus effected that enables the ray-flower to produce a large corolla; and this again we may conjecture to serve, by the brilliancy it gives to the capital, to attract insects.* The disk-flowers by this arrangement are free from any obligation to produce a showy corolla, and so have more material at their disposal for a plentiful production of pollen.

This view of ray-function is in harmony with the fact that, as a rule, in non-radiate capitals the individual florets are larger and more conspicuous than the disk-flowers of a radiate. They have to perform for themselves the duty which in the other case falls almost exclusively on the ray. It harmonises also with the fact, that when either there is an exception to this rule, or when in radiates the ray is so small as to add but little to the brilliancy of the capital, other means are usually adopted to increase the attractiveness. Thus a number of the inconspicuous capitals may be massed into a corymb or a panicle, which, in virtue of increased size, appeals more fully to the eye. Such, for instance, is the case with *Eupatorium*. Or the flowers may be endowed with a strong aromatic odour, as in *Artemisia*, and insects be thus allured through another sense than sight. Or both plans may be combined, as is the case with Tansy. Again, what function has the ray, if not that here assigned to it? Perhaps it may be said that it has none; that it is nothing more than the inevitable but useless consequence of the staminal suppression; that the primary object of Nature was to produce a monœcious condition, which is of known advantage;

* That the ray serves to attract insects was held by Sprengel, as I learn from "Origin of Species," p. 145. Mr. Darwin "does not feel at all sure that the idea is so far-fetched as it may at first appear."

and that the production of the ray was merely a secondary result of this, the spare material necessarily appearing in some shape or other. But this view is irreconcilable with the fact, that in some capitals (e.g. *Centaurea Cyanus*) the suppression includes not only stamens but pistils. Here there is a large unhandsome ray, and yet no monœcious condition. What possible interpretation can be given of such a ray of neutral flowers, excepting that it is intended to attract insects?

We have seen that in such flowers as Feverfew self-fertilisation must not be an infrequent occurrence, though less frequent than intercrossing. In some other nearly allied flowers—for instance, in the Marigold—it is altogether prevented. For here, not only do the ray-flowers cease, as in Feverfew, to produce pollen, but the disk-flowers develop no stigmas. It is curious to notice what modifications this further step entails. The pistils of the ray, as before, are without unnecessary tufts of hair and bear their stigmas on a bifid extremity. The disk-flowers have their anthers as usual united into a tube: now, were the pistil altogether absent, there would be no means of bringing the pollen to the surface. The style, therefore, remains with its terminal brush; but as there are no stigmas this tufted extremity no longer requires to be divided. It is therefore simple and not bifid.

The two capitals I have described are from one group of composites (*Seneciοids*). In the other groups the main facts are the same: that is to say, the anthers in all form a tube from which the growing style brushes out the pollen; in all the pollen is spread on the surface before the stigma of the same flower is mature, and in all there are collected in one head flowers in different stages, so that in all intercrossing by aid of insects must be the rule. But the position and arrangement of the hairs on the style which sweep out the pollen differ in each group; and it is this difference which furnishes the characteristic mark of each subdivision.

I pass on to one of the most attractive of irregular forms—the Papilionaceous. The whole purport of this is to ensure the transfer of the pollen to an insect, which is the same thing as to ensure repeated intercrossing. I will take as an example the common furze (*Ulex*). The upper petal—standard—requires little notice. It serves, by its size and brilliancy, to add to the attractiveness of the flower, and also as a protection to the more important parts below. The two lower petals are united together to form a kind of boat—carina—in which are lodged the pistil and stamens. The comparison to a boat is rather far-fetched, for not only is the casina closed below in the part which would represent the keel, but also above; so that the organs within are close prisoners. Were the upper edge of

the carina not thus united, the style and stamens would not remain in the cavity; for their elasticity tends to make them spring upwards, so that they are always pressing from within against the upper side of the carina. Consequently, if a flower be gathered and the carina be gently opened above with a needle, the pistil and stamens will be seen to spring from their prison. The same result will ensue if simply downward pressure be made on the carina, for this is so united to the axis as to admit easily of depression. Should, then, a bee settle on the upper surface of the carina, the weight will tend to depress this. This will assist the upward pressure of the organs within, and if the weight be sufficient they will break through the cohesion of the petals which opposes them and spring upwards, while the carina will sink downwards. In the mature flower the weight of a bee is often insufficient to effect this; either the cohesion of the petals being stronger or the upward pressure of the style stamens being weaker than at a later period. So that one may frequently see a bee visit a just opened flower without liberating the reproductive organs. Probably these are not then in that state of maturity which would render their liberation advantageous. But if a flower be mature, the first time a bee visits it the carina is depressed, the stamens and pistil fly upwards and strike the under surface of the insect with some force. A little cloud of pollen is seen to be thrown off by the shock, some of which will adhere to the insect, and cannot fail to be frequently carried to another flower, while the rest is scattered over the neighbouring flowers, in many of which the stigmas are already exposed by previous visits of bees.

I have spoken as though the bee settled directly on the carina. In reality it settles not on this, but upon the side petals—alæ—the legs of the one side of its body upon the one, those of the other side upon the other. These alæ afford a better landing-place than would the narrow edge of the carina. The weight, however, still acts upon the carina as much as if the bee had settled directly upon it. For there are, on the outer surfaces of the carina and on the inner surface of each alæ, certain projections and recesses which fit into each other, so that alæ and carina are locked together, and depression of the former implies necessarily depression of the latter.

In many species of *Leguminosæ* this jointing together of alæ and carina is replaced by actual coherence. Such, for instance, is the case with the Sweet-pea, Scarlet Runner, &c. In some species the upward spring of the stamens and pistil when set free from the carina is even more marked than in *Ulex*—as in Broom, or still more conspicuously in *Medicago*. In most, however, no such motion can be observed. In these cases the contact of pollen and stigma with the bee's body is brought

about simply by the descent of the carina, the reproductive organs themselves being entirely passive. As these have no tendency to spring upwards, there is no occasion for the upper edges of the carina to be united together. Such an arrangement would indeed interfere with easy depression. The edges, therefore, are either left ununited in their whole length, in which case the descent of the carina leaves the stamens and pistil fully exposed; or they are united for some distance, but a fissure is left between them at the distal end, through which pollen and stigma protrude during the depression. What the special objects of these variations may be I do not profess to know. The same general purposes are, however, plainly discernible in each case. These are—the protection of the reproductive organs at ordinary times from wind, rain, cold, and other noxious influences, and their exposure, at the moment of a bee's visit, in such a way as to ensure their contact with its body.

In the generality of Papilionaceous flowers it is the under side of the bee which is struck by the anthers. A striking exception is furnished by the French Bean (*Phaseolus comm.*) and the Scarlet Runner (*Phas. coccineus*) (fig. 4). The carina in these flowers is closed, excepting at the very end where the stigma projects very slightly from a round opening. It is adherent to the alæ, not merely jointed with them. Instead of lying between them in a more or less horizontal position, it rises up vertically, and is coiled into a spiral form. This spiral is such that the opening at its end lies just above the only passage by which access can be had to the nectary. On looking at a flower in front (fig. 4), one sees this passage on one's left; and one sees that there is no open passage on the right, the carina being so placed as to block it up. A bee, then, in order to get at the nectar, must push its head and thorax directly underneath the opening from which the tip of the stigma is protruding (fig. 5). While doing this its weight acts through the alæ, on which it stands, upon the adherent carina, which it depresses. The terminal opening is thus brought close to the bee, while the carina is stripped from the style—on which the weight does not act—and leaves a considerable length of this with the stigma uncovered, and in contact with the back of the bee. Judging merely by the length of the stamens, one would expect that the same movement which exposes the style would also expose the anthers, for these lie round the style only just behind the stigma, that is, round a part which the movement does lay bare. But the style is stout and resisting, whereas the filaments are slender and limp, and not firm enough to resist the friction of the retiring carina, with which they are therefore drawn back. The anthers, as they retreat, are of course rubbed against the stationary style. This is set with dense hairs, point-

ing towards the terminal opening (fig. 6). These hairs answer to the so-called *poils collecteurs* in the composites: they sweep off the pollen, so that when the style is protruded its exposed portion—which alone is hairy—is seen thickly covered with the grains. None, however, are seen on the stigma; for, the style being longer than the stamens, this lies beyond the anthers, and the movement only separates them still more. The viscid surface of the stigma is moreover turned away from the anthers, so that any grains which may drop from them accidentally will not light upon it.

The weight of the bee, then, causes a viscid terminal stigma, succeeded by a pollen-smeared style, to protrude from the carina, and both come into contact with the upper surface of the insect. The stigma strikes it first, and as the bee pushes deeper into the corolla the viscid surface is rung along its body from before backwards, and will inevitably carry off any pollen which may be there. For not only is the stigma viscid, but it is set round with a brush of fine hairs, which assist in collecting the grains. These are truly *poils collecteurs*, and must be carefully distinguished from the longer hairs on the style, which, as I have already said in speaking of the composites, would more fitly be called *poils expulseurs*. The surface which has been swept by the stigma immediately receives a fresh supply of pollen from the style which follows closely behind, and thus the bee leaves the flower in a condition suitable for the fecundation of the next which it visits.

Both humble and hive bees fertilise this Bean. Most of them have, however, learnt to get at the nectary feloniously by making a hole in the tube. In other similar instances it has always seemed to me that there was an evident reason why the bee should not go in at the natural opening. Either its proboscis was too short, or its head too large. But in this instance no such explanation is possible; for while some bees visit the flower in the natural way, others of the very same species and of the same size avail themselves of the shorter cut, which involves less effort on their part. Whenever I have watched an individual bee visiting a succession of Bean flowers, I have always seen it visit them all in the same way. If it entered the first by the mouth, it continued to do so throughout; if by the artificial opening, it kept persistently to the same plan. It would thus appear that the habit is not an instinct, belonging by inheritance to the whole species, but is in each case the result of individual experience. As with the same experience some bees have acquired the habit and others have not, we must admit not only that these insects are intelligent, but that they differ from each other in their degrees of intelligence, some being slow in acquiring knowledge, others quicker.

The Scarlet Runner is one of the very few plants on which I have found time and opportunity to experiment. Having covered a large portion of a plant with gauze so as to exclude the visits of bees, I found that out of a vast number of blossoms thus protected not a single one produced a pod, while the unprotected blossoms were for the most part fruitful.* Even of unprotected blossoms a very considerable proportion failed to produce pods—a much larger proportion, I think, than is the case with most flowers. This is probably due to the fact already mentioned, viz. that most bees have learnt to get at the nectary by nipping the tube. I have more than once watched for a considerable time without seeing a single bee visit the flowers in any other way. Were all bees equally clever, there would be an end of Scarlet Runners; unless indeed either nature or artifice were to induce some modification of structure—such as the large calyx of *Pedicularis*—by which the tube might be protected, and the bees again driven to the mouth.

Very similar to the Papilionaceous flowers are the Fumitories, as regards the mechanism for their fertilisation. Here again, two of the petals are partly united so as to form a receptacle, in which are lodged the reproductive organs (fig. 7). The arrangement of the other two petals is such that an insect, in order to get at the nectary, is forced to light on this receptacle, which is expanded laterally so as to form a convenient landing-place. When the insect is so placed, its weight causes the receptacle to sink, and the reproductive organs issue from their hiding-place and strike the insect on its under surface. Here also the emergence of the organs is due sometimes simply to the descent of the receptacle, and sometimes is assisted by a tendency of the elastic style and stamens to spring upwards.

The next and last flowers I shall deal with are some in which the anthers alone are irregular. Everyone knows the cross-leaved Heath (*Ericatetralix*), so common on boggy mountains. This has a corolla (fig. 8) which may be roughly described as bell-shaped, and which hangs when expanded, as a bell should, with its mouth more or less downwards. The style will stand for its clapper, but with this difference, that it reaches to the very mouth of the bell, or even projects slightly, when it terminates in a semiglobular stigma. The viscid surface of this

* A similar experiment, however, which I made on the Kitchen-garden Pea (*Pisum sativum*) gave a very different result. The protected flowers formed pods as well as the unprotected. The Pea, then, is capable of self-fertilisation, without the aid of insects. This, however, does not exclude the probability of more or less frequent intercrossing; and it seems to me impossible to suppose that the Pea should possess, without any purpose, a mechanism which in other Papilionaceous flowers, as *Phaseolus*, &c., has a distinct use.

faces downwards, that is, away from the cavity of the bell. The style rises from a superior ovary, under which is a disk that secretes a fluid highly attractive to all kinds of bees. From beneath this spring eight stamens, which soon converge and end in a circlet of anthers set closely round the style (fig. 8). Each anther consists of two cells, adherent in their lower and middle parts, slightly divergent at their apices. From the base of each cell a curious flat process is thrown out almost horizontally, towards the inner wall of the corolla, which it nearly touches. Thus when one looks into the mouth of the bell one sees sixteen processes radiating from the centre like the spokes of a wheel, and forming an imperfect barrier between the upper and lower halves of the bell. The other ends of the two cells of each anther, as already said, diverge slightly, and near the apex of each, on the external lateral aspect, is an oval opening or pore (fig. 9), which gives issue to the pollen. As the whole corolla hangs upside down, the pollen grains would at once fall out from these pores as soon as dehiscence occurred, were there nothing to prevent it. But even then it is clear that the grains would not light on the viscid surface, for this faces in the wrong direction. The pollen, however, is prevented from so falling; for each anther cell adheres just in the part where its opening is situated to the corresponding part of the adjoining cell of the next placed anther in the circlet. Thus the pore of a cell, say the light cell of an anther, is, so to speak, closed by the pore of the left cell of the next adjoining anther; and so on all the way round. Thus the pollen is kept imprisoned in the cells by their mutual adherence. A very little force, however, is enough to dislocate this chain of anther cells. If a slight pressure be made down upon one of the radiating processes, which form the long arm of a lever of which the filament is the fulcrum, the upper ends of the corresponding anther cells, which form the other arm, are lifted up, and break from their union with their neighbours. The pores are then disclosed, and the pollen grains fall out in obedience to gravity. When a bee visits a flower it pushes its head against the mouth, which is too small to admit it, but which it obstructs completely. In so doing the head necessarily strikes against the viscid stigma, and will leave on this any pollen grains that it may have. The bee then extends its proboscis down to the disk to suck the nectar. The proboscis can scarcely get there without striking one or other of the processes, and as soon as it does this the jointed chain of anther cells, as before explained, is dislocated, and a shower of pollen falls from the exposed pores on the insect's head. This the bee carries of, and will, of course, give up to the stigma of the next flower it visits.

In our botany manuals the structure of the individual anther

is always minutely described, but no mention is made of the all-important fact of the adherence of the contiguous cells of neighbouring anthers in their dehiscing portions. This, however, is easily seen if one selects a young flower which has not yet been visited, and removes the corolla with due care. Actually to see the bee strike the processes is of course impossible; but I have often seen one come to a flower with a clean head, and leave it with a head dusted with pollen grains.

In the fine-leaved Heath (*Erica cinerea*) there is a mechanism for fertilisation precisely similar to that of *Tetralix*. Probably the same is the case with all those Heaths in which the anthers are provided with processes.

In the closely allied *Vaccinium* there is a somewhat different though very similar arrangement. Here also the anthers are furnished with processes, which act as in the Heaths. But the pores at the farther end of the cells are not placed on the sides, as in the Heath, but are actually terminal (fig. 10). They cannot therefore be closed as before by lateral adherence of contiguous cells. The closure, however, is effected in another way. The apex of the cell where the pore is placed presses against the style, so that the style itself serves the office of a cork, and prevents the escape of the pollen. The filament is so bent as to act like a spring, and keep the anther firmly pressed against the style (figs. 10 and 11). It is only when pressure is made upon some of the processes that the corresponding anther is tilted up, its terminal pores exposed, and the pollen allowed to escape. This pressure is applied, as in the Heath, by the proboscis of the bee. The result, as before, is a shower of pollen upon the insect's head, which it carries off and leaves upon the slightly projecting stigma of the next flower it may visit.

In the common *Arbutus* the mechanism of the anthers is similar to that in *Vaccinium*—that is to say, the pores are blocked up, while the flower is undisturbed, by the style itself. The arrangement is shown in fig. 12, which represents the position of the parts in a mature flower. In fig. 13 the same parts are shown in a yet unopened flower-bud. It will be noticed that the position of the anthers is exactly reversed in the two figures. In the bud the processes of the anthers and the parts in which the pores will afterwards appear are turned away from the mouth of the flower. In the mature blossom they are turned towards it. The anther, therefore, has undergone a revolution, has been turned upside down during the ripening of the blossom; and it is plain that if it had not done so, but had retained the position it held in the bud, the pollen could not have fallen out of the cells, seeing that the mature flower hangs with its mouth downwards.

The way in which this revolution of the anther is brought

about appears to me to be as follows. In the bud the sharp-pointed end of the anther (fig. 13) adheres to the style, with which it is in contact, by means of a sticky, gum-like fluid. Sometimes the anther-point does not seem to be in actual contact with the style, but to be united to it by a slender white thread (fig. 13). This thread, however, I take to be some of the same gum-like fluid, which has been accidentally pulled out and then has dried. Be this as it may, in some way or other the point of the anther is at this period attached to the style, and therefore fixed. The other end of the anther is jointed movably to the filament. Consequently, as the growing filament increases in length, a bend necessarily occurs at this joint, the lower end of the anther being tilted up, and the anther made to rotate round its fixed end. This motion continues till the anther reaches the position it holds in the mature flower (fig. 12), when the opposing style hinders any further rotation. Such lengthening of the filament as may occur after this can only take place by the filament itself curving outwards; and the more it is thus curved, the more firmly will it press the anther against the style, and the more securely, therefore, will the pollen be retained. No pollen escapes during the rotation, for the pore remains closed until it comes into contact with the style.

It may seem strange that Nature should have adopted so apparently roundabout a plan for bringing the anthers into their necessary position. Why should they not have been developed from the first inverted, and with their pores in contact with the style, in which case no rotation would have been requisite? In answer to this, it may be said that Nature by no means invariably selects the most direct path to reach her end, but, as the student of development well knows, frequently proceeds in a zigzag in the production of an organism; and not rarely, after following some definite path for a certain distance, may even retrace her steps, obliterating as she does so her footprints, and start afresh in a new direction.

The flowers with which I have now dealt in this and two former articles are numerous, and belong to many different orders. In all we have seen that there are special arrangements to ensure the transfer of pollen to the body of an insect, which is the same thing as saying that there are special arrangements to ensure at least occasional intercrossing.* If to these be added the Orchises, *Tynums*, *Tythrums*, *Primulas*, &c., &c., which Mr. Darwin has long since shown to be dependent upon insects for their due fertilisation, and to these again such other instances as have been described by other observers, we have

* See "Popular Science Review," July 1869.

a vast array of facts telling all the same tale, that it becomes impossible to refuse assent to the Darwinian generalisation, "Nature abhors perpetual self-fertilisation."*

EXPLANATION OF PLATE LIX.

FIG. 1. *Feverfew.* A central flower just expanded: (*a*) anther-tube closed.

FIG. 2. *Feverfew.* A flower somewhat less central: (*a*) anther-tube broken open, and the pollen escaping.

FIG. 3. *Feverfew.* A still more peripheral flower. The style projecting from (*a*) the anther-tube; (*c*) the stigmas exposed; (*b*) tufts on extremities of style.

FIG. 4. *Phaseolus coccineus.* Front view, (*a*) standard; (*b*) right ala, pulled somewhat aside; (*c*) left ala, cut off at point where it adheres to carina; (*e*) vertical carina; (*d*) terminal opening of carina, with tip of stigma protruding.

FIG. 5. *Phas. cocc.* Lateral view. Letters as in fig. 4.

FIG. 6. *Phas. cocc.* End of style and stigma, highly magnified: (*a*) stigma with hairs round it; (*b*) long hairs of style; (*c*) opening at end of carina.

FIG. 7. *Fumitory.* The two outer petals cut off. A and B the two inner petals, united at C and expanded laterally at E into a landing-place; F the fissure through which the reproductive organs issue; D the diadelphous stamens and the style.

FIG. 8. *Erica tetralix.* The filaments not represented excepting cut ends of two at *d*; (*c*) ovary; (*e*) hypogynous disk; (*b*) style surrounded by circlet of anthers; (*a* and *a*,) these letters point to the processes of one and the same anther. So also do (*b*) and (*b*). The cell to which *a* belongs coheres with the cell to which *b* belongs externally, in the part removed farthest from the processes.

FIG. 9. *Erica tetr.* Single stamen: lateral view; (*a*) processes; (*b*) filament, cut short, (*c*) pore.

FIG. 10. *Vaccinium myrt.* Two stamens *in situ*, showing how the terminal pores are set against the style.

FIG. 11. *Vacc. myrt.* A single stamen: (*a*) terminal pores; (*b*) filament; (*c*) processes.

FIG. 12. *Arbutus.* Two stamens *in situ* in mature flower; their pores against the style.

FIG. 13. *Arbutus.* Two stamens *in situ* in bud: (*a*) the closed pores. The stamen on the left is attached by its sharp end to the style by a thread.

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* The various mechanical contrivances described in the preceding paper have never, so far as I know, been described before. But my acquaintance with botanical literature is next to nothing, so that it is quite possible that such descriptions may have been given repeatedly without my knowing it. Should such be the case, my ignorance must be my apology for passing over any previous observations without notice.

THE TURRET-SHIPS *MONARCH* AND *CAPTAIN*.

By S. J. MACKIE, C.E.

[PLATE LX.]

WITHIN the last few months considerable attention has been attracted to the subject of sea-going rigged turret-ships, and the performances of the first two vessels worthy this title—the *Monarch* and the *Captain*—have been viewed with the greatest interest. There were, it is true, previously in our navy several turret-ships, but of these the *Royal Sovereign* and the *Prince Albert* had scarcely any pretensions to sail power, being really coast-defence vessels: while the *Scorpion* and *Wyvern*, the famous Birkenhead rams, although masted and rigged, could not be trusted at sea without a convoy. The latter vessels, it will be remembered, were purchased in 1865 from their builders, Messrs. Laird; and soon afterwards, in the early part of January 1866, the Admiralty ordered the *Monarch*. The *Captain* was commenced about a year later. Both the vessels are now completed, the *Monarch* having been in active service for some months; the *Captain* is almost ready for sea, and has been tried at the measured mile, besides making the passage from Birkenhead to Portsmouth. It will be interesting, therefore, to compare the principal features of these two fine vessels, as they have little in common beyond the turret armament, and as so much has been said at various times respecting the advantages and disadvantages of the two types they represent.

The *Monarch* represents the high freeboard type of turret-ship, and was designed at the Admiralty by Mr. E. J. Reed, the chief constructor of the navy; the *Captain* represents the low freeboard type, and, as is well known, was designed by the Messrs. Laird, under the direction of Captain Coles, the Admiralty having thus put into the hands of that gentleman the power of giving the fullest expression to his ideas—a power for which he had often asked. Under these circumstances, the *Monarch* must be regarded as the Admiralty model for a rigged turret-ship; and the *Captain* as Captain Coles' model vessel, he

having doubtless received considerable assistance in working out his plans from the eminent firm to whom the construction of the ship was entrusted.

The *Monarch* (Pl. LX., fig. 1) presents externally an appearance closely resembling that of a broadside iron-clad. Her sides rise to the height of 14 feet above water, and she has a complete upper deck and forecastle, on which the greater part of the ropes are worked, just as in a broadside vessel. Her armour is also disposed on a plan identical with that of our recent broadside ships, a belt (indicated by the black portion in the figure in the plate) protecting the region of the water-line throughout the whole length, and a central battery or chamber constructed amidships. On the belt the armour rises to the height of about 4¾ feet above the water; while, on the central battery, it rises to the full height of 14 feet. At the bow and stern also the armour is carried up above the belt, in order to form protected batteries for bow and stern guns, or, as they are technically termed, "chasers." The lower edge of the armour belt is five feet below water. In the central protected chamber on the main deck the turrets are placed, their top parts projecting about 6½ feet above the upper deck, the engines for turning them being placed on the lower deck beneath their bases. By this arrangement the turret-guns are carried at a height of about 16 feet above the water. The high freeboard of the vessel has been adopted in order to ensure this great height of the guns above the water, for, in consequence, the capability of the ship to fight in a sea-way has been largely increased.

Turning to the *Captain* (Pl. LX., fig. 2), we find an entirely different arrangement. The upper deck proper is only a few feet above water; the original intention was to have had a freeboard of 8 ft., but this has been decreased to about 6½ ft., by causes which will be hereafter referred to. The hull is armoured throughout the length to the height of the upper deck; and the lower edge of the armour was intended to be about 5 ft. below water. The turrets stand upon the lower deck, and project about 6½ ft. above the upper deck, the height of the turret-ports above water being actually about 8 ft. only, instead of the originally intended height of 10 ft. This statement makes it evident that in fighting power in a sea-way the *Captain* must prove inferior to the *Monarch*, as her ports have only about half the elevation above the sea, whatever the relative capabilities of the two vessels may be in other respects. The most important feature in the *Captain* remaining to be noticed in this general view, is the "flying deck," which extends from the poop to the fore-castle at a height of about 11 ft. above the upper deck, and to which access is obtained by means of a large deck-house, or casing, placed between the turrets. This

deck is about 25 ft. wide, and on it are placed the bitts and other gear usually fixed on the upper deck of a sailing ship. In fact, the working of the ship will be conducted from this deck; and on it will be stowed the boats, spare spars, and gear usually carried on the upper deck. The *Monarch*, as has been said, has a complete upper deck of the usual kind; and the principal difference between her and broadside ships is, that the boats are carried on a small flying deck, extending from the foremast to the main-mast, in order to leave the deck free for the fire of the turret-guns, while the shrouds are arranged in an unusual manner for the same purpose. The *Captain* has tripods in the place of lower masts and shrouds.

Passing on from this general description, it will be proper to compare more closely the dimensions and other particulars of the two ships. The *Monarch* is 330 ft. long, 57½ ft. broad, and has a deep-load draught of about 24¼ ft.; her burden being 5,099 tons. The *Captain* is 320 ft. long, 53¼ ft. broad, and has a burden of 4,272 tons; her mean draught of water, when fully equipped, was intended to be 23 ft., but it will really be more than 25 ft. When tried on the measured mile recently, her actual draught exceeded the estimated load draught by 22 in., and as she then was only in the condition of a ship stored for a complement of 400 men and officers, while her actual complement is to be 500, she is likely to sink several inches more, thus decreasing her freeboard still further. This miscalculation is very greatly to be regretted, as it is a serious feature, inasmuch as it reduces the ship's fighting capabilities, and makes the work of propulsion so much the heavier. The causes of this increased immersion have been mainly due, without doubt, to the attempt to do more on the dimensions than could be done in combination with such structural arrangements as have been adopted on the hull; and as the result, we have the actual weight of the ship exceeding the estimated weight by not less than 800 or 900 tons—something near one-ninth or one-tenth of the total weight of the ship. We have heard more than once of the greater results obtained in the *Captain*, on smaller dimensions, as compared with the *Monarch*; but it should be remembered, that though the tonnage of the *Captain* is 800 tons less than that of the *Monarch*, her load-displacement is only about 400 or 500 tons less, and her mean draught of water about a foot more.

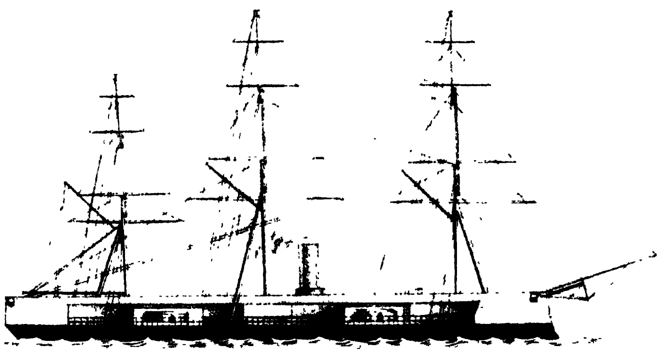
Keeping these facts in view, let us pass on to notice the armour and armaments of the two vessels. The *Monarch's* broadside is protected with 7-inch plates, 12-inch teak backing, and an inner skin of 1½-inch iron, supported by strong vertical and longitudinal frames, or girders. The protection of the *Captain* is of about equal strength, except in the wake of the

turrets, where the side armour is increased to 8 inches instead of 7-inch, as in other parts. The turrets of both vessels are of nearly equal strength—namely, 8-inch plates—except at the ports, where 10-inch plates are used. This armour is supported by 10 or 11-inch backing, and $1\frac{1}{4}$-inch skin-plating in the *Monarch*, and 1-inch skin in the *Captain*. The low upper deck of the latter vessel, being exposed to injury from the depressed fire of an enemy, has also to be protected; and for this end is covered with $1\frac{1}{2}$-inch or 1-inch plating. The *Monarch's* upper deck obviously does not require similar protection, being so much higher. This condition also renders any jamming of the turrets much less probable in the *Monarch* than in the *Captain*.

The turret armaments of both ships are of equal power, and consist of four 25-ton 600-pounder rifled guns. But in both the range of the guns in a horizontal direction—that is, technically, their "arcs of training"—is limited. The original idea of a turret-ship was that she would possess "all-round fire"—in other words, that her guns could be pointed at an enemy in any and every direction; but the necessity of protection from the weather or from the sea—in short, the fitting the turret-ships for ocean-going purposes, has induced modifications which very largely restrict this essential quality, and leave now but very little difference between the utmost extent of the horizontal range of the guns in turret and broadside ships. The poop and forecastle of the *Captain*, for example, prevent the turret-guns from being trained nearer to the fore and aft directions than angles of 23 degrees forward and 25 degrees aft. In the *Monarch* a similar limitation occurs. The latter ship, however, possesses absolute fore and aft fire in the $6\frac{1}{2}$-ton guns (115-pounders), carried as bow and stern chasers in the protected batteries previously referred to; while the *Captain* has no fore and aft fire from "protected guns"—a very important deficiency in an armoured ship—although she does carry two similar $6\frac{1}{2}$-ton guns on the forecastle and poop, which would be of service at long ranges. The "all-round" protected fire which the *Monarch* in this way possesses, and her greater capacity for fighting in a sea-way, obviously render her superior to the *Captain* in offensive powers; and although the *Monarch* may be slightly inferior to her competitor in the strength of some portions of the side-armour, she is, on the whole, a more efficient war-ship. She is, of course, larger and more costly; but she is also more commodious and more speedy, and these qualities are most important in fighting ships.

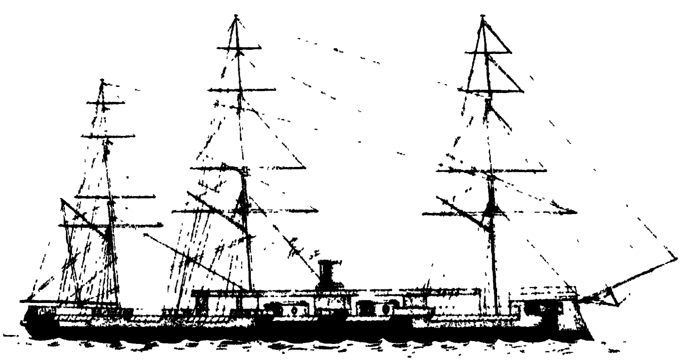
The *Monarch* is the fastest iron-clad yet built. She is driven by engines of 1,100 horse-power nominal, working up to about seven times, and on her measured mile trial made very nearly

1



H M S Captain

2



H M S Monarch

...kie del. W. West lith.

The two new Turret Ships

Scale ... [illegible]

15 knots (14·937) per hour, while on the six hours' trial at sea she made nearly 14¾ knots (14·715). The *Captain* is also a fast vessel, although not nearly so fast as the *Monarch*. Her engines are of 900 horse-power nominal, and on the measured mile trial worked up to about 6¾ times, driving the ship at a speed of 14·239 knots. The *Monarch* is a single-screw ship; the *Captain* has twin screws.

Both vessels are fully rigged and equipped for sailing. The *Monarch* has had full trial at sea during the cruise of the squadrons last autumn, and her recent trip with the remains of Mr. Peabody across the Atlantic, and has proved herself a very fair performer. In fact, from the accounts which have recently been published of her voyage to America, it appears that she is fast under canvas as well as under steam. The *Captain* has not yet been tried at sea, but she will probably answer very fairly under sail. One other feature of the *Monarch's* behaviour, which has been prominently brought out by trial, is her remarkable steadiness. During her cruise last autumn, she is said to have been fully capable of fighting her guns in weather when all the other iron-clads were unable to fight. The *Captain* is reported to have behaved very well on her passage to Portsmouth, but she has yet to be tested at sea, and is not likely to equal her rival in steadiness.

From these facts it appears that both the *Captain* and the *Monarch* are very formidable vessels, and that they form important additions to our armoured fleet. In armour and guns they are superior to the greater number of our iron-clads, and in speed they also stand high. As compared with each other, there seems no question that the *Monarch* is the more efficient; and though she is more costly than the *Captain*, her greater efficiency as a war-vessel makes ample amends. In view of the success which she has achieved in steaming, sailing, and other respects, it is clear that the assertions of defects, made before her trial, were unfounded; neither can it now be maintained that the turret-system will not have in her as fair a trial as in the *Captain*. Indeed, for the high freeboard type, the *Monarch* will stand on her merits; whilst excuses for shortcomings in the *Captain* will be admissible for the low freeboard type.

In conclusion it is worth pointing out, that in the figure of the *Monarch* in the plate, some sections of the bulwarks, or technically "tumble-down top-sides," are shown up, and some turned over, to give play to the fire of the turret-guns; their extent shows the full capacity of training. In the *Captain* the deck sides are protected only by stanchions and ropes. Both drawings are accurately made to scale, namely $\frac{1}{104}$ of an inch to a foot; and an exact comparison of these two turret-ships can thus be made.

REVIEWS.

FARADAY AS A MAN AND AS A PHILOSOPHER.*

THERE are very few men, even of the philosopher class, who would like to have the story of their lives told by the publication of their own private correspondence. For it happens, that not many, even of the wise, are devoid of pettiness of character, and the smaller and less creditable traits of their mind and disposition would be sure to unfold themselves in their common-place letters to their friends and relations. But Faraday was not made of "such stuff." Those who knew him felt that there was a character of constancy, so to speak, about the man, so that he was as much the philosopher at his own fireside as when he fascinated the crowded audiences in the Institution by the grandeur, and yet simplicity, of the laws which he had revealed. Some of our philosophers are, we are sorry to say, savants in the lecture theatre, and the veriest prigs in common life; men full of meanness, jealous of their successful brethren, exacting a stern submission from those who have not yet earned a reputation as great as theirs; men, in fact, who pursue the study of Nature not for itself, but for their own honour or glory, and for the mere gratification of a particular kind of vanity. But Faraday was a man as little to be compared with men of that stamp as Hyperion to a Satyr. All gentleness, kindness, and consideration in his dealings with his fellows, he loved his pursuits from the sheer pleasure which he found in solving the many puzzles which he saw around him. He felt himself in rivalry with none, he encouraged all to work in the vast field he cultivated so well, and, so far as he could, he assisted and protected the efforts of those whom he knew to be honest in their researches; and if his indignation was ever roused, it was when his aid was sought by some one who wished to obtain his name in support of charlatanism.

Need we be surprised, then, that the task which Dr. Bence Jones has so well and lovingly discharged in the two fine volumes before us, is one which shows us in Faraday an almost god-like nature, which his mere physical researches

* The "Life and Letters of Faraday," by Dr. Bence Jones, Secretary to the Royal Institution. 2 vols. Longmans, 1870.
"Faraday, as a Discoverer," by John Tyndall. New edition. Longmans, 1870.

could not have indicated? Dr. Jones has collected together all the letters of Faraday (or nearly all) from the time when the little errand-boy had become a pupil of the great Sir Humphry Davy, to the letter which, in July 1866, he dictated and signed to Dr. Jones, in sending him his second edition of the Works of Shakespear. It would be out of the question to do justice to this noble work in our brief space, for it is only by the assistance of many quotations from the touching, tender, truth-breathing letters of Faraday, that we could give the reader an adequate idea of the, in this age, rarity, thoroughly Christian character of Faraday's life. Suffice it, then, to say, that both the general and scientific public are immensely indebted to Dr. Jones for the good work he has done in publishing Faraday's letters, and in thus telling the tale of the good and kind old man, as it were, in the very words which were so dear to all who knew him.

The book which is included in the second part of this notice is also a biography of Faraday, but it is a work of quite a different character to the other, and one, too, which must be read, as it supplies that part of Faraday's history which his letters but very imperfectly describe. Dr. Jones's volumes are really a sort of autobiography. Dr. Tyndall's smaller volume is a picture of Faraday as a scientific man. It is the laboratory life of the great discoverer, and it is a sketch executed with feeling and power. We have already noticed its merits in these pages, and we need do no more now than mention, that in this new edition, which forms a sort of appendix, as it were, to the "Life and Letters," the author has corrected a statement in reference to Faraday's and Ampère's experiments, of some importance. Dr. Jones's volumes are full of illustrations, and Dr. Tyndall's contain two excellent engravings of Faraday, taken from photographs. These two are standard works, and no educated person, who can afford to purchase them, should be without them.

EARTH AND SEA.*

WE should certainly feel that something had gone wrong with the literary-scientific world, if the quarter came round without bringing us some luxuriously "got up" compilation from the pen or pens of M. Louis Figuier. Fortunately or unfortunately, as it may by different sides be regarded, this quarter is not exceptional. There is now before us one of those *éditions de luxe*, which have lately become so popular, and behold! M. Figuier is its author. "Land and Sea," however, is a work which, perhaps owing to the exertions of a conscientious and enterprising editor, has more claims to importance than most of M. Figuier's treatises. It is a volume covering 700 pages of the largest 8vo. It bristles with illustrations, there being a handsome woodcut on nearly every page, and finally, its type, paper, and binding combine to make it a drawing-room book of no ordinary beauty.

But its scientific merits are really of a high order too, so that we think

* "Earth and Sea." From the French of Louis Figuier. Translated edited, and enlarged by W. H. Davenport Adams. London: Nelson & Sons, 1870.

the publishers are entitled to credit for the enterprise they have shown in introducing so good a work to the notice of English readers. The subject discussed by the author is physical geography, and as this is a branch of science which may be said to have undergone little or no serious change during the past ten or fifteen years, it is easy to comprehend that M. Figuier's usual hap-hazard mode of compiling has stood him in better service than when he has applied it to other more progressive departments. In this volume we are treated to an account of the general physical features of the globe, and of the laws which tend to give it its existing form, and to keep it in its present place in the solar system. The work opens with a pleasantly written sketch of the geography of the ancients, from the time of Genesis down to the period of Columbus. Then follow the six books into which the whole subject-matter is divided, and the headings of which are as follow: The situation of the terrestrial globe in space; the form and dimensions of the terrestrial globe; the surface of the globe; the temperature of the globe; the fresh waters; and lastly, "the world's seas."

In this way the history of the mighty influences which are, even now, shaping the globe to its future form, are successively discussed, and by a number of exquisite engravings, some of them a little idealised, the reader's interest is awakened in and his attention is concentrated on the author's account. The chapters devoted to the history of the Arctic regions are among the most forcible and graphic in the whole work, and the illustrations are many of them novel, and all most striking and picturesque. The book, taken as a whole, is really a good one.

ASIATIC CHOLERA.*

IN the whole range of Ætiology, there is perhaps no disease, whose natural history is at once so interesting and yet so unsatisfactorily established as that of Cholera. We do not mean what is sometimes in this country termed English Cholera, for that is a disease which is in no way epidemic, and which is really but a particular form of colic; but the Asiatic Cholera, that terrible pest, which, when it reaches these climates, slays at least thirty per cent. of those it attacks, and which, in the East, attains a virulence and destructiveness, which only those who have seen strong men struck down in a single hour can realise. What is the nature of this terrible plague; how did it first originate; and by what means is it propagated and distributed from its chief centre?—these are the problems which, despite the splendid treatise which Dr. Macnamara has given us, are still unsolved by Scientific Medicine. A great approach, however, to a philosophic solution of these points is made by Dr. Macnamara, who, in the 550 pages of which his work consists, not only gives us an able analysis of the current doctrines, but supplies us with the facts of his long, ripe experience as an Indian doctor.

Not only to the physician, but to the general scientific man, and es-

* "A Treatise on Asiatic Cholera." By C. Macnamara, Surgeon to the Calcutta Ophthalmic Hospital. London: Churchill, 1870.

pecially to the microscopist interested in the now popular germ theory of disease, this book of Dr. Macnamara's will be found full of interest. Doubtless many of the author's opinions will differ from those of his readers, but all will admit that, while putting forward his own ideas very clearly, Dr. Macnamara does no injustice to the ideas of his fellow-labourers in the field of medical science. Every theory and doctrine has its claim fairly discussed ere it is rejected, and we trust that the author's notions, founded on a very careful practical and literary study of the entire subject, will meet the attention they deserve. The author sums up his opinions in a series of Aphorisms, which he styles the characteristics of the disease. We have not space to reproduce all these, but we may observe that two of them, supporting as they do the general opinion of medical men in this country, are of special importance. One of these is to the effect, that the disease is never originated *de novo* in an uninfected district by any condition we are acquainted with; the other point is, that the disease is contracted by the passage into the body of some material derived from the intestinal canal of the affected person.

As appendices to Dr. Macnamara's work, there are a number of Reports on various Indian outbreaks, and an account of Dr. F. Macnamara's army filter, a contrivance which bids fair to be of much use in the prevention of cholera. Our best thanks are due to the author for his labour.

GEOLOGY AND REVELATION.*

IT is with much pleasure that we note, from the Catholic side of the Christian church, a tendency to consider the statements in Genesis in a broader light than that in which they were viewed by the older divines. The present work has been written by the Professor of Theology to the Maynooth College, and its aim is pretty much the same as that of the works of Hugh Miller, Dr. Pye Smith, and others. Dr. Molloy tries to prove, that the account given in Scripture of the creation of the globe is not to be taken in the literal sense of seven days; but is to be regarded as implying possibly thousands of years, or a certain number of epochs. His method of argumentation is a twofold one. He cites numerous passages from the writings of the Fathers, to show that such an extended interpretation of the Bible is not out of accordance with the views of theologians. Then he enters on a general popular sketch of Geology as a science, and shows that, by the record of the rocks, the world, as we see it now, is not the result of operations carried on during only seven days. Thus far it will be seen that Dr. Molloy goes no further than those who went before him, in this particular work of reconciliation between science and the Scriptures. He tells us that the Biblical chronology did not commence till the creation of man, and that, dating from this time, it is absolutely correct. We of course do not differ from so erudite a theologian as Dr. Molloy on questions of this kind, but we doubt, if the chro-

* "Geology and Revelation; or, the Ancient History of the Earth considered in the light of Geological Facts and Revealed Religion." By the Rev. Gerald Molloy, D.D. London: Longmans, 1870.

nology of the Bible is to extend to no more than six thousand years, how the author will explain away the troublesome fact of the antiquity of man, which researches prove to extend to some two hundred thousand years. However, as Dr. Molloy does not touch on this point in the present work, but promises to go into it in a future one, we must only await the appearance of his next essay. His "Geology and Revelation" is a well-written, able, and scholarly work, and one which will be read with interest by men of all persuasions.

THE ANTHROPOLOGICAL MEMOIRS.*

DESPITE the ferocious onslaughts of Mr. Hyde Clarke, the Anthropological Society still flourishes, and in accordance with its usual custom, has this year published a fine volume of Memoirs. Of course, some of the papers printed in this work are not so good as others, and there are some which hardly merited being printed at all; but then we must bear in mind that a Society often accepts papers having little intrinsic interest, which are, nevertheless, likely to excite a discussion at the meetings, and that such papers must in due course be printed with the rest. There are certain contributions, worthy of careful reading. Of these is one by Dr. Shortt, on the "Bayadères or Dancing-girls of India." This is a very curious paper on an equally curious subject, and is handled in a thoroughly scientific spirit by the author. Another memoir of particular import, is that of Mr. W. Bollaert, on "Ancient Peruvian Graphic Records." Sir Duncan Gibb also contributes a couple of good articles, and Dr. Beddoe, the President, publishes a very lengthy and valuable essay, on the "Stature and Bulk of Man in the British Isles." Besides the contributions, there are several very well-executed plates of Skulls, and other illustrations of interest. The publication of such a volume as the present is the best *raison d'être* that the Anthropological Society can offer its opponents.

MEDICAL ELECTRICITY.†

THE first edition of Dr. Althaus' treatise was a small but very well and favourably-known volume; and, so far as we can see, the second edition is likely to become a standard work in the therapeutics of electricity. This edition differs from the old one, in many important particulars. It is a very much larger work, we should think three times the size of the old one. The additions are very numerous, and relate chiefly to electro-physiology generally. We presume the author has tried to make the work as complete as

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* Memoirs read before the Anthropological Society of London, 1867-8-9. Vol. iii. London: Longmans, 1870.

† "A Treatise on Medical Electricity, Theoretical and Practical, and its Use in the Treatment of Paralysis, Neuralgia, and other Diseases." By Julius Althaus, M.D. Second edition. London: Longmans. 1870.

possible, so that the student might find within its covers all that is of importance in relation to the effects of electricity on the body. Dr. Althaus first gives an account of the different forms of electricity (employing, we are sorry to see, the objectionable word "fluid" throughout); then he states the results of the researches of Matteucci, Dubois Reymond, and others in the phenomena and laws of electro-physiology; next he describes the different varieties of medical electric appliances, and their modes of application; and, finally, he gives the history of a number of cases cured by electricity. The volume covers nearly 700 pages, and is literally crammed with illustrations. Dr. Althaus has not simply compiled; he has exerted a certain amount of criticism, and is occasionally severe on those whose opinions differ from his own. Still his treatise is not the less useful for that, and we have pleasure in commending it to our readers.

THE PROGRESS OF MEDICINE.*

NEVER was the *parturiunt montes* proverb more applicable than in the case of the work which is now before us. Dr. Dobell attempted a task far too vast even for the College of Physicians to undertake. He apparently had little or no experience of the method of preparing abstracts of the progress of science, and as a result we have a big volume without any satisfactory plan or arrangement, in which a multitude of facts, many of them not worth narrating, are jumbled together higgledy-piggledy, so that it is quite impossible, without considerable labour, to ascertain what is said to have been done on any special subject. Dr. Dobell's scheme appears to have been to write to a number of persons in various parts of the world, without very much discrimination as to their ability for the duty he asked them to perform, to send him their ideas of the progress of medicine in their particular locality. Some of these correspondents replied to him, and hence we are bored with a series of uncompressed, diluted communications, which have no value whatever to the scientific man. Here and there are a few abstracts of interest, but they are rare, and very difficult to disinter from the huge mass of rubbish which constitutes the bulk of the volume. We have never had the misfortune to read a book with so little scientific character about it; and we can only hope, for the sake of the reputation which English science has obtained abroad, that this book may not be considered in any way representative of English medicine. Dr. Dobell promises us future volumes, but we shall be very much surprised if he does not find the "scientific and practical" medical men of this country too much dissatisfied with the first volume to venture to invest in the second. We are sorry thus to have to speak of what was, at all events, well-intentioned as an effort, but which has certainly been, in our opinion, a lamentable waste of time and type.

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* "Reports on the Progress of Practical and Scientific Medicine in different parts of the World (for the Year beginning June 1, 1868, and ending June 1, 1869)." Edited by Horace Dobell, M.D. London: Longmans.

THE STUDY OF INSECTS.*

ONCE more, but not too frequently, we have to offer our thanks to Dr. A. S. Packard, for his admirable treatise on Insects. Parts VIII. IX. and X. have just reached us, and are what we might have anticipated from the excellent character of the preceding portions of the work. They conclude the volume, and contain the index and preface, thus completing a treatise which only those who have seen can appreciate. The account of the *Coleoptera* almost terminates in Part VIII., and the *Hemiptera* are commenced in the Ninth Part, which also embraces a considerable portion of the description of the *Orthoptera*. Part X. contains the history of the *Neuroptera*, and also an account of the *Arachnida*, which Dr. Packard, for various developmental reasons which he distinctly states, regards as being unquestionable insects. In these Parts there are some very capital page-plates, and the woodcuts scattered through the text are both numerous and good. There are two points about this work which render it superior to any other treatise on Entomology in the English language. One of these is the fact, that the author gives a description, accompanied by figures, of the fossil types of each group. The second is a peculiarity to which we referred in former notices, but on which we cannot dwell too forcibly; it is the fact that Dr. Packard goes, with the most thorough detail, into the development of each Order. Not only does he give what he has seen in his own numerous observations, but he compiles his additional facts from the most eminent recent European and American authorities on Insect Embryology. Dr. Packard's work is, we say it unhesitatingly, the best we have ever seen.

WHAT ARE THE STARS?†

THIS is a handsomely bound and abundantly illustrated, but we would add, unprofitable little volume. The author, or authoress, has not been very careful to make herself acquainted with modern astronomy, and is, moreover, not very exact in her mode of expression, nor, we venture to think, well adapted to teaching the young. Clearly, the little work she has written is intended for young people, and yet we find the authoress telling her young readers about ellipses and parabolas, as though they were the commonest terms of ordinary nursery conversation. When we further state that, according to this work, the sun is inhabitable and derives its heat and light from some other source, and that the planets move round the sun nearly in circles, we have given some notion of its merits. We need say no more in its favour.

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* "A Guide to the Study of Insects, and a Treatise on those injurious and beneficial to Crops, for the use of Colleges, Farm Schools, and Agriculturists," by A. S. Packard, Junr., M.D. Salem, U.S. Parts VIII. IX. and X.

† "What are the Stars? or a Treatise on Astronomy for the Young," by M. E. Storey Lyle. London: Sampson Low, 1870.

A YEAR-BOOK OF FACTS.*

MR. TIMBS' "Book of Facts" comes out annually, with a considerable stock of interesting matter, and in the number which recounts the doings of 1869, it is not behind its predecessors. Nevertheless, it is nothing more than what its name implies. It is a book which contains many facts in relation to the year's scientific progress. It is divided into a number of sections, corresponding to the several branches of physical and natural sciences, and under these it supplies us with cuttings from a limited number of English periodicals. Indeed, if we were to erase from its pages the extracts from the *Illustrated London News*, the *Athenæum*, and the *Mechanic's Magazine*, we should have very little matter remaining. We note also, that the compiler has exercised little judgment or discretion, and that he has aimed rather at filling his book than at the selection of facts which really, in each division, represent the actual progress of science. We are sorry to observe, too, that in some instances the source of the "cutting" is not stated, although to the scientific man its origin is at once apparent. Not the least important part of the volume is the obituary notice at the end. This contains not a few errata; but, on the whole, it is well compiled. The engraving, or frontispiece, represents the present Constructor of the Navy, Mr. Reed.

SECONDARY FOSSILS AT CAMBRIDGE.†

A TROUBLESOME but very useful labour has been performed by Mr. H. G. Seeley, in preparing a Catalogue of the *Aves*, *Ornithosauria*, and *Reptilia* of the Secondary System, which are in the Woodwardian Museum of Cambridge. Only those who have had to wander from case to case in a large Museum, in utter despair of ever finding the specimen they were in search of, can realise the time spent by Mr. Seeley in preparing this Catalogue, and the great benefit that the book will confer on working palæontologists. The plan of the Catalogue is similar to that of the collection it describes, and is so convenient, that a student wishing to ascertain and see what specimens of any particular genus the Museum possesses can do so with very little loss of time or trouble. We cannot but concur in the words of the venerable Geologist who has written the preface to this work, when he says, "Of the value of the work the readers of the Catalogue—those especially who read it in our Geological Museum with the arranged specimens before them—will be the best judges; and that it will be of great use to the Academic student I cannot for a moment doubt. He may see the specimens, one by one and side by side with the printed lists, and be enabled in that way to

* "A Year-Book of Facts in Science and Art, &c.," by John Timbs. London: Lockwood, 1870.

† "Index to the Fossil Remains of *Aves*, *Ornithosauria*, and *Reptilia*. From the Secondary System of Strata arranged in the Woodwardian Museum of the University of Cambridge," by H. G. Seeley, with preface by the Rev. A. Sedgwick, LL.D., F.R.S. Cambridge: Deighton, Bell & Co.

comprehend the evidence on which the several arrangements are founded, and thereby he may be led to grasp the anatomical reasoning of a more detailed Catalogue, which will I trust soon follow." Judging from the present effort, we join heartily in Professor Sedgwick's hope.

THE INDUCTIVE METHOD IN RELIGION.*

It is not often the scientific reviewer's fate to meet a work on science and religion in which the general exactness of science, and the temperate language which should characterise religion, are to be found united. Generally speaking, the works of this class are rude fulminations against the arrogance of man's philosophy, and, as such, they have little influence in bringing about the end for which they are intended. Happily, however, in the ably-written and honestly-worded work which Dr. Gladstone has prefaced, and Mr. Miller has written, we find a book which even the philosophical infidels of science will receive, and read with respect, however much they may dissent from the ideas which it puts forward. The author's notion, as very clearly explained by Dr. Gladstone, is, that just as the inductive method is the basis of, at least, the natural sciences, so might its aid be sought in support of Christianity. The Bible, which he takes it for granted is revealed, contains a large mass of facts, and if these be properly collated, and then inductive reasoning be brought to bear on them, the only logical result will be some form of religion similar to that of the Christian world. This is fair enough, so far as it goes, and is, after all, pretty much what Butler means in his 'Analogy,' for argument by analogy is an essential element in inductive reasoning. But we think the author should have gone somewhat further than he has done; and, as he sought to educe religion by the inductive plan, he should have commenced, as Butler does, by trying to show, inductively, the extreme probability of a Creator making a revelation to the creature. In other respects, the argument, throughout the work, is most logical, and will, we doubt not, convince many.

Reliquæ Aquitanicæ. By Edouard Lartet and Henry Christy. Edited by Professor Rupert Jones. Part X. London: Baillière. 1870.—This Part of this sumptuous treatise was issued in February, and is chiefly occupied with M. Lartet's able essay on "The Employment of Sewing Needles in Ancient Times." The plates are numerous, and contain, as usual, beautiful and truthful lithographs of bone implements, flint weapons, and some of the peculiar needles referred to in M. Lartet's account.

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* "Christianum Organum; or, the Inductive Method in Science and Religion." By Josiah Miller, M.A. With an Introduction by John Hall Gladstone, Ph.D., F.R.S. London: Longmans, 1870.

The Philosophy of the Bath. *By Durham Dunlop, M.R.I.A.* Dublin: Moffat and Co.—This is a one-sided and rather dull account of the benefits which the hot-air bath has conferred on mankind. Mr. Dunlop does not appear to have any real acquaintance with either the progress or the requirements of modern scientific therapeutics.

Education and Training considered as a subject for State Legislation. By a Physician. A Re-issue. London: Churchill. 1869.—We have already, in its earlier edition, expressed our opinion of this interesting work.

SCIENTIFIC SUMMARY.

ASTRONOMY.

Gold Medal of the Royal Astronomical Society.—At the General Meeting of the Royal Astronomical Society, on February 11, the gold medal was presented to M. Delaunay, for his researches into the problem of the moon's motion.

The Imperial Observatory at Paris.—The same astronomer, well known as a skilful mathematician, has been appointed to the management of the Paris Observatory, in the place of M. Leverrier. The latter had given offence to his colleagues by the brusqueness of his demeanour and the difficulties which he threw in the way of independent scientific research. Loewy, Wolf, Marié Davy, and Villarceau, *astronomes adjoints*, drew up a memorial, which was signed by all their subordinates, in which they addressed the Minister of the Interior, pointing out the defects in the superintendence of the Observatory, and laying stress on special grievances to which they had been subjected. They submitted to him the choice of dismissing Leverrier or accepting their resignations. He adopted the former alternative. If Delaunay is a less distinguished astronomer than Leverrier, as cannot indeed be questioned, it is yet certain that under his superintendence better work will be done at the Paris Observatory. All astronomers will, however regret that the name of Leverrier, which has outshone all but a few in the galaxy of astronomical fame, should thus be temporarily clouded. Doubtless by fresh claims to the admiration of the scientific world he will, before long, rise stronger from his fall.

Colour Changes in the Planet Jupiter.—We have mentioned, that last October Mr. Browning noticed that the great equatorial belt of Jupiter, usually the brightest part of the planet's disc, was of a greenish-yellow tint, resembling the colour known among artists as yellow-lake. Since then the belt has passed through other changes, appearing sometimes of an almost full orange-yellow, at others coppery-red, while its boundaries both on the north and south have exhibited the most surprising changes of figure.

New Micrometer for Measuring the Position of Lines in Faint Spectra.—Mr. Browning has contrived a bright-cross micrometer, by which the lines in faint spectra may be very conveniently measured. A small tube attached to the side of the star-spectroscope has at its outer part a glass plate, blackened, with two fine cross-lines across the centre. A lens produces an image of the bright cross in the field of view, by reflection at the nearest face of

the compound prism. On turning the micrometer screw, the slide which holds the glass plate travels in grooves, and the illuminated cross is thus made to traverse the whole length of the spectrum. It may be suggested, that perhaps a single short line, at right angles to the direction of the spectral lines, might serve as well as the cross-lines, if only its centre were marked with a dark point. By such an arrangement the dark line to be measured would not, however faint, be obliterated near its centre, as when the angle of the cross-lines falls there.

Application of Photography to the coming Transits of Venus.—Mr. Proctor's paper on this subject, in the Monthly Notices for January, points out the advisability of combining certain pairs of stations, so chosen that the parallactic displacement of Venus as seen from those stations may be on a radial line of the sun's disc. In all other cases, he remarks, an error of importance may be expected to accrue from errors in superposing the fiducial lines of two photographs which are to be compared. Where the displacement is on a radial line there is no necessity for superposition, since the mere measurement of the planet's distance from the centre of the sun's disc will afford the necessary information. The large plate which illustrates Mr. Proctor's paper has been very skilfully engraved by Messrs. Malby and Sons.

The New Eye-piece for correcting Atmospheric Dispersion.—A very simple arrangement for getting rid of the flint prisms proposed by Mr. Airy, without introducing optical defects, occurred independently to the Astronomer Royal and to Mr. Simms. It consists merely in making the eye-glass—plano-convex—broader than is strictly necessary for telescopic vision, causing it to press by its convex surface into a concave cup at the eye end of the eye-piece, and allowing it to roll in that concavity; thus presenting different parts of its convex surface, though always in the same form and position, to the rays of light which come from the field-glass, but presenting to the eye a plane surface, which, in one position of the lens, is normal to the telescopic axis, and in other positions is inclined to it at different angles.

New Theory of the Milky Way.—Mr. Proctor has been led, by a careful examination of the structure of various parts of the Milky Way, to the conclusion that the true figure of the system of stars constituting this zone can neither be that of a cloven disc, as supposed by Sir W. Herschel, nor that of a broad flat and in part cloven ring, as suggested by Sir John Herschel. He points out that the existence of round *coal-sacks* in the Milky Way is as conclusive as to its figure, at least in those parts, as the round figure of the Magellanic Clouds is as to the general figure of those strange clusters. We cannot suppose the coal-sacks to be tunnel-shaped openings extending through the whole breadth of a wide flat ring, without the "obvious improbability" spoken of by Sir John Herschel when dealing with the supposition that the nubeculæ may be cylindrical in figure. This being so, it follows that if the coal-sacks are really openings through a star-zone, that zone cannot, in all probability, have a much greater extension in the direction of the line of sight than at right angles to that line. According to this view the section of the Milky Way near the coal-sack in Crux (and presumably elsewhere) would be, roughly, circular. And viewing the Milky

Way as a ring of circular section—that is, as resembling in section an ordinary wire ring—one can understand many peculiarities of its structure which seem wholly opposed to either the disc or the flat-ring theory. For example, the great gap in the constellation Argo may be readily explained, and so also can the yet wider vacant space in the fainter branch where the ring is double. Mr. Proctor shows how, by assigning to the Milky Way a spiral figure, nearly all the principal peculiarities of the zone can be very fairly accounted for.

The Sun's Corona.—Astronomers are already looking forward with interest to the total eclipse of next December, when they hope to solve the perplexing problem presented by the solar corona. Mr. Lockyer, in a paper communicated to the Royal Society, has expressed his continued adherence to the theory which explains the corona as due to the glare of our own atmosphere. In this way he gets over certain difficulties presented by the results of spectroscopic analysis as applied by himself to the chromosphere and prominences, and by the American observers to the corona. It may be questioned, however, whether these spectroscopic observations cannot be interpreted more simply. One point, at any rate, is obvious; the corona is not a solar *atmosphere*: that it is, however, a solar appendage, can hardly, we think, be reasonably doubted.

The November Meteors.—Observations made on these objects, last November, have continued to come in from various far distant stations. They point conclusively to a well-marked spreading of the meteor system, as compared with the portion through which the earth passed in November 1868. In fact, from some observations made by Lieut. Tupman at Port Said, it would seem as though the width of the system had increased fully fourfold in the interval. The problem presented by the November meteors becomes more and more interesting, the more we consider the relations really involved in what has been discovered respecting the extent of the system.

Mr. Carrington's new Observatory and Horizontal Alt-Azimuth.—Mr. Carrington, whose researches into solar physics must be familiar to all our astronomical readers, has just completed the construction of a new observatory which presents several features of interest. The observatory is on the summit of a hill sixty feet high, but is itself sunk below the ground, "just peeping over the soil." Mr. Carrington has sunk a dry well, six feet in diameter, to the depth of forty feet, from the centre of the observatory, and with a horizontal shaft communicating with the south side of the hill, 166 feet in length, closed with three doorways. This is chiefly meant for the clock, for Mr. Carrington is "determined to have one clock at least properly mounted, at a position of invariable temperature, and in an air-tight case." The principal telescope is an alt-azimuth, constructed on a new principle. Mr. Carrington has adopted Steinheil's principle of making the horizontal axis the effective optical axis, by placing the object-glass at one end and the eye-piece at the other, with a prism outside the object-glass. The casting of the prism took over three months, but Messrs. Chance succeeded at last. "It is some comfort," remarks Mr. Carrington, "to think that never need the telescope be raised, only turned round, and the observer always under cover."

The Proper Motion of the Stars, and the Sun's Motion in Space.—Mr.

Proctor, as we have already mentioned, has recently obtained some rather singular results respecting the proper motions of the stars, so far as they avail to indicate the motion of the sun through space. His paper on the subject has since appeared in the Monthly Notices of the Astronomical Society. It will be remembered that Mr. Dunkin, applying a method devised by Mr. Airy to the motions of 1,167 stars, found for the point on the heavens towards which the sun's motion is directed a place closely corresponding with the mean of results obtained by others. But when he applied to the uncorrected sum of squares of the stellar proper motions a correction corresponding to the deduced solar motion, he found, in place of a considerable reduction, a sum not differing by one-thirteenth from the uncorrected sum. Mr. Proctor, analysing Mr. Dunkin's results according to several distinct hypotheses, arrives, by independent methods, repeatedly to this one conclusion, that the distances of the fainter stars have been largely over-estimated by astronomers. Finally, dividing the whole number of 1,167 stars into two sets, one including the stars of 1st, 2nd, and 3rd magnitudes, the other those of the 4th, 5th, and 6th magnitudes, and comparing the mean distances inferrible from the mean proper motions of each set, he finds the ratio absolutely one of equality. It would follow, of course, that the stars of the lower orders of magnitude are relatively small (instead of being very far from us), and that in such a degree as to enable us to refer these orbs to the same region of space as the larger stars. Mr. Proctor remarks, however, that he is far from wishing to place so great a stress on his results; but he considers that they do suffice to render the usually accepted views respecting stellar distribution wholly untenable.

Star-drift.—In a paper communicated to the Royal Society, Mr. Proctor points out another peculiarity of the stellar proper motions. In certain regions of the heavens the stars exhibit a well-marked tendency to drift in a definite direction. Mädler had already noticed this in the case of stars in the constellation Taurus, but the German astronomer was mistaken in supposing that the drift in this constellation is exceptional. On the contrary, there is a more remarkable drift in the constellations Cancer and Gemini; while in many regions of the heavens the drift is at least as remarkable as in Taurus. It is to be noticed, therefore, that whatever stress has been laid by astronomers on Mädler's conclusion that Alcyone is the centre of the stellar motions is misplaced, since a similar community of motion is observed in other neighbourhoods. One of the most singular instances of star-drift is recognised in the constellation Ursa Major. The five conspicuous stars β, γ, δ, ϵ, and ζ are all travelling in the same direction and at the same rate, in a direction which is exactly contrary to that due to the stellar motion. If these stars indeed form a single system, and it is scarcely possible to draw any other conclusion from so remarkable a community of motion, the mind is lost in contemplating the enormity of the cyclic period of this vast system. The duration of our solar system must be regarded as a mere moment by comparison.

Summary of Sun-spot Observations made by means of the Kew Photo-Heliograph during the Year 1869.—The following table exhibits the annual *résumé* of observations made at Kew, drawn up according to the plan of Hofrath Schwabe, of Dessau:—

Months	Days of Observation	Days without Sun-spots	Number of New Groups	Numbers in the Kew Catalogue	
January	14	0	15	No. 902 to	916
February	15	0	17	917	933
March	11	0	14	934	947
April	20	0	15	948	962
May	16	0	18	963	980
June	18	0	27	981	1007
July	22	0	18	1008	1025
August	19	0	25	1026	1050
September	21	0	21	1051	1071
October	18	0	17	1072	1088
November	11	0	15	1089	1103
December	11	0	22	1104	1125
	196	0	224	902 to	1125

There is a steady increase in the number of groups and the areas covered by them, a circumstance which points to the approaching maximum of the sun-spot period. The observers remark that the year was characterised by a remarkable tendency of the groups to appear in successive trains, within narrow and well-defined zones, on both sides of the solar equator. A regular successive appearance of spots along parallels of latitude had been previously observed, but it usually only lasted during a short period, after the lapse of which the distribution in latitude became again irregular. Last year the irregularity of distribution was the exception. It is not improbable, the observers add, that a distinct law may be traced at some future time in this singular behaviour, and they recommend the subject to the attention of observers.

Professor Kirkwood on Sun-spot Periods.—This laborious and thoughtful astronomer has subjected Wolf's periods to careful scrutiny, and has been led to the conclusion that in order to account for sun-spot periods we must suppose that portions of the sun lying in certain solar longitudes are more capable of being influenced by disturbing causes than other regions. He ascribes to Mercury the most powerful disturbing effect, and in particular he regards this planet as the cause of the 11-year period—46 revolutions of Mercury being equal to 163 solar rotations, and to about $11\frac{1}{14}$ years.

Comets and Meteors.—The same astronomer shows reasons for believing that the solar system, as it passes through the interstellar spaces, traverses regions in which cometic or meteoric matter is sometimes densely and sometimes sparsely strewn. He concludes that during the interval from 700 to 1,200 the solar system was passing through or near a meteoric cloud of very great extent; that from 1,200 to 1,700 it was traversing a region comparatively destitute of such matter; and that about the commencement of the eighteenth century it again entered a similar nebula of unknown extent. He points to a fact which has not hitherto, so far as we know, been noticed, that all the comets whose perihelion distances are less than 0·01 have their perihelion close to the direction towards which the sun is moving, while those whose perihelion distances are less than 0·05 exhibit a well-marked approach to the same peculiarity of distribution.

The Planets.—Saturn will be the only planet favourably situated for observation during the coming quarter. His ring is now open to its widest extent. But the planet will be low when on the meridian.

BOTANY.

The Professorship of Botany in the Irish College of Science.—This chair has been filled by the election of Professor Dyer, late of the Royal Agricultural College, Cirencester. The choice of Mr. Dyer gave rise to great dissatisfaction among Irish scientific men, it having been thought that either Professor Perceval Wright, who had especial claims, or Dr. M'Nab, the eminent vegetable histologist, should have been preferred to the Cirencester Professor. However, now that the matter is decided beyond dispute, we have no doubt that Mr. Dyer will discharge his duty with ability and distinction Mr. Dyer has already done some good work in botany. The appointment to the Dublin College leaves his chair at Cirencester to Dr. M'Nab.

The Geographical Distribution of the Coniferæ.—Mr. Robert Brown has sent us a reprint of his valuable paper on this subject, published in the Transactions of the Botanical Society of Edinburgh. We have not space for all the author's conclusions, some of which are of especial interest. In regard to the influence of temperature on the distribution of *Coniferæ*, he thinks that this is not so important as moisture, which latter, he thinks, the all-important element in the growth of forests; the great steppes of Asia and prairies of America being almost entirely due to the absence of sufficient moisture for the prosperity of trees. Trees require at least from 15 to 16 in. of rain during the growing season. The dense forests, for instance, of the western slope of the Cascades are wholly due to the abundant supply of rain that region obtains. Its effect is well seen in the treeless aspect of San Diego, in Southern California, which has an annual rainfall of only 8½ in., while Sitka, in the territory of Alaska, is deluged under the enormous rainfall of nearly 7½ *feet* per annum!

Heather a Productive Plant.—One would hardly think the growth of heather could be made commercially profitable, and yet M. Dromard, who writes on the subject in the *Annales du Génie Civil* of February, is of this opinion. In France, and in a great many other European countries, very large tracts of land are uncultivated and left waste, producing nothing but heather and similar plants. The author's lengthy memoir describes practical methods whereby these plants are employed for the production of charcoal, acetic acid, tar, and other useful products, and the soil so cleared as to become fit for the cultivation of various kinds of fir-trees.

Cinchona Growth in India.—At the meeting of the Linnæan Society, on the 3rd of March, Mr. J. E. Howard read a paper by Mr. Broughton, chemist to the cinchona plantations in the Madras Presidency, "On the Hybridisation among Cinchonas." He believes that the sub-varieties of *Cinchona officinalis* are permanent, but that hybrids can be artificially obtained, although they do not occur in nature. The cinchona has long been known to belong to the class of dimorphic plants. In the discussion

which followed, Dr. Anderson, superintendent of the Botanic Gardens at Calcutta, gave some interesting particulars of the cultivation of cinchona at the Darjeeling plantations. The culture of the plant, too, in St. Helena is progressing satisfactorily. The plants are all in excellent health, and have a fine green, vigorous appearance. There are now about 4,000 planted out, and it is thought a sufficient number can be obtained from them to stock the whole colony.

Barleria Lichtensteiniana.—Under this title Dr. T. Maxwill Masters describes a new garden plant, in a recent number of the *Gardeners' Chronicle.* The characters are described by Dr. Masters with much detail, and of its general features he gives the following description: "It is one of the most peculiar-looking plants we have ever had the fortune to see; and though it is destitute of the attractive colours which render so many of its fellow *Acanthads* such favourites in our stoves, it is very far from inelegant. The structure of the flowers, and the arrangements apparently intended to prevent cross-fertilisation and to secure self-fecundation, invest it with additional attractions for the plant lover."

The Chemistry of Lichens.—At the meeting of the Royal Society, on the 24th of February, Dr. Stenhouse read a paper "On the Organic Compounds in certain Lichens." The species were *Cladonia rangiferina*, and a mixture of *Usnea barbata* and *Evernia prunastri.*

The Flora of Naples.—In January Professor Allman, of Edinburgh, paid a visit to Naples, and sent the following account of the flora to the Botanical Society of Edinburgh. (February 10th.) "It is the indigenous flora, after all, which, in my opinion, gives a special and peculiar charm to the Mediterranean vegetation, and possesses an interest beyond that of all the introduced plants, however beautiful. It is on the coast road from Naples to Amalfi that we have the native flora in perfection, for, though still rather early in the year for its full development, the hill-sides are clothed with *Arbutus*, and arborescent heath and myrtle, and *Cistuses* and rosemary wherever the native vegetation is not displaced by terraces of lemons, and oranges, and olives. The *Smilax* clings wherever it can get support; the woods are filled with purple crocuses, and the broad leaves of the *Cyclamen* give promise of a carpet of flowers somewhat later; the little Italian *Arum* is in blossom in every shady nook, and wherever there is a moist rock the beautiful fronds of the *Adiantum capillus-veneris* form a tapestry of exquisite verdure. Those who have seen only the flora of Northern and Central Europe can have no conception of the wonderful richness and beauty of the vegetation of the warmer temperate zone.

The Physiology of Floating Leaves.—In a recent number of the *Botanische Zeitung* Herr Hildebrand gives an account of some interesting observations in the physiology of the floating leaves of *Marsilea quadrifolia*, which is thus well abstracted by one of our contemporaries:—When a plant of this species is sunk beneath the surface of the water, so that all the leaves are more or less deeply covered, those leaves which are fully developed at the time of immersion, remain unchanged, while those which are not so far advanced, undergo a remarkable change; the petioles gradually lengthening in succession according to their position on the stem, and soon over-topping those which were already formed. At first the four

leaflets do not increase, but they soon begin to enlarge, and by the time the surface of the water is reached, they exceed in size the ordinary leaves, forming a four-rayed star on the surface. While the petioles of the ordinary leaves are stiff, so that they stand erect out of the water, these floating leaves are weak and flexible, like those of water-lilies, allowing the leaf to maintain its position on the surface with the rise and fall of the water. Their upper surface is shining and coated with wax, so that the water flows off them. If immersed in deeper water, the petioles will lengthen still further, even to the extent of three feet. In these cases the formation of the organs of fructification appears to be suppressed. In the ordinary aerial leaves, stomata are found on both sides of the leaf in about equal numbers; in the floating leaves, on the other hand, the under side is entirely destitute of stomata, while on the upper surface they are about three times as numerous as in the aerial leaves; thus resembling *Nymphæa*, *Hydrocharis*, and other plants.

Icicles in the cells of Plants.—At a meeting of the Academy of Sciences of Paris, on 21st February, M. Prillieux sent in an interesting paper on the congelation of plants. He has established the existence normally of large icicles in the interior of all frozen plants. These icicles form small columns, perpendicular to the surface, and often penetrating the epidermis. The ice is formed from liquids derived from the cells. The cells themselves remain intact, so that there is no destruction, but simply a separation of organs, and therefore what has been said concerning the death of plants by freezing goes for nothing.

The Acrogens of Lake Superior, America.—Mr. Macoun, of Belleville, lately returned from a somewhat extended botanical tour around the north shore of Lake Superior, which occupied him during July and part of August. By dint of excessive work, he has made a large collection, many of his specimens being of great rarity and interest. A catalogue of all the plants noticed and collected by him is in progress, and will probably be published by instalments in the *Canadian Naturalist*. The Acrogens, being worked up, are given as a beginning in the last number of that Journal, and the opportunity to include the species obtained by other collectors, in the same locality, is availed of.

Geography of Pinus Pungens.—In a note to a paper on "Variations in *Pinus* and *Taxodium*," recently published by the Philadelphia Academy of Natural Sciences, Mr. T. Meehan has given another locality for them: "on the hills north of Harrisburg, along the Susquehanna," and he states that they are probably abundant through the centre of the State.

The Colouring Matter of the Alder.—Dingler's *Polytechnisches Journal*, in its second number for January, gives a paper on this subject by Herrn Dreykorn Reichardt, which is thus given in abstract in the valuable scientific summary of the *Chemical News*:—It is a well-known fact that the wood of the *Alnus glutinosa, L.*, when recently cut, exhibits a series of colourations, rapidly changing from yellow to reddish-brown. The authors isolated this colouring matter, which is perfectly insoluble in ether, benzol, and sulphide of carbon, difficultly soluble in absolute alcohol and boiling water, but readily soluble in dilute alcohol in every proportion. The different tests applied to this substance resulted in defining it as a peculiar

tannin material; with gelatine solution it is precipitated, with chloride of iron it yields a green precipitate, and its combinations with the heavy metals are insoluble in water; its formula is $C_{54}H_{28}O_{22}$. This material yields, when split up by the action of sulphuric acid, sugar, and a peculiar reddish-brown-coloured resinous substance, insoluble in water and ether, difficultly soluble in alcohol, readily soluble in caustic soda solution and in ammonia, from which solutions it is precipitated again by acids.

Fossil Botany in 1869.—The first number of the *Journal of Botany* contains, besides various other papers of much interest, a very important communication from Mr. W. Carruthers, of the British Museum, entitled, "Review of the Contributions to Fossil Botany published in Britain in 1869." The author gives some account of all the papers of the year, and a list of the genera and species referred to.

The Structure of a Fern-stem.—At the last meeting of the Geological Society of London, Mr. Carruthers, who has the field of Fossil Botany almost exclusively to himself in this country, read a paper "On the Structure of a Fern-stem from the Lower Eocene of Herne Bay, and on its allies, recent and fossil" The author described the characters of the fossil-stem of a fern obtained by George Dowker, Esq., F.G.S., from the beach at Herne Bay, and stated that in its structure it agreed most closely with the living *Osmunda regalis*, and certainly belonged to the *Osmundaceæ*. The broken petioles show a single crescentic vascular bundle. The section of the true stem shows a white parenchymatous medulla, a narrow vascular cylinder interrupted by long slender meshes from which the vascular bundles of the petioles spring, and a parenchymatous cortical layer. The author described the arrangement of these parts in detail, and indicated their agreement with the same parts in *Osmunda regalis*. He did not venture to refer the fern, to which this stem had belonged, positively to the genus *Osmunda*, but preferred describing it as an *Osmundites*, under the name of *O. Dowkeri*. The specimen was silicified, and the author stated that even the starch-grains contained in its cells and the mycelium of a parasitic fungus traversing some of them were perfectly represented. Its precise origin was unknown: it was said to be probably derived from the London clay, or from the beds immediately below.

A Giant Fir.—*Cosmos* of March 5 states that there has been just cut down, at Arwa, in Hungary, a fir tree, 139 ft. high, and 71 inches in diameter at 2 ft. from the soil. This tree, as regards size, is a specimen now very rarely met with in Europe, though more common to America.

Death of Mr. Sowerby.—Since our last issue, science has had to record the decease of Mr. F. J. E. Sowerby, whose beautiful illustrations of British plants, and especially those which have appeared in the *English Botany*, which Mr. Hardwicke is now publishing, are so well known to lovers of plants. It was fortunate, for the sake of the handsome and valuable work which Mr. Hardwicke is publishing, that all the drawings were completed before Mr. Sowerby's decease.

Professor Unger, whose papers on the relation between the existing Flora of New Holland and the Eocene Flora of Europe, were so ably epitomised some years ago in these pages by Dr. B. Seemann, has quite recently died. He appears to have died suddenly, and under suspicious circumstances. It is

reported that a Styrian priest alleges that the body of Professor Unger was probably destroyed by the devil himself, to whom his soul belonged. Vegetable Palæontology has sustained a severe loss in the death of this able botanist.

Intercellular Substances and Cuticle.—Dr. W. R. M'Nab, some twelve months ago, read a paper before the Botanical Society of Edinburgh, on the Staining of certain Vegetable Tissues. In this he took occasion to remark, that the cuticle covering the external parts of plants could be readily stained with carmine, the so-called intercellular substance remaining colourless. During the year these experiments have been frequently repeated, and always with the same result, namely, that while the cuticle was quickly and deeply stained with carmine, the intercellular substance remained colourless. Careful observation of the growth of cells in the young roots of the white mustard (*Phalaris canariensis*), and the garden pea and bean, have led to the conclusion that the so-called intercellular substance is in reality the original or primary cell-wall—that as growth goes on this primary cell-wall becomes thickened by the addition of numerous more or less marked layers on the inside. In the stems of many plants it requires some care to be able to demonstrate that the cell-wall and thickening layers are separate. In the layer of cells of the epidermis, on which the cuticle rests, the outer surface is in general greatly thickened, while the inner part of the cells is only thickened at the angles at which the other cells join. This thickening at the angles is often so great that the cell appears almost filled up, or the thickening appears as a continuous layer. In the epidermal and subepidermal cells of the ivy the thickening is so great that, without careful examination, the thickening might be considered continuous. On the more or less thickened external surface the cuticle rests, and may be considered as thickening occurring outside the cell-wall. There are many examples of this thickening outside a cell-wall, the covering being analogous to a cuticular layer. As in the extine and intine of the pollen grain, the intine representing the primary cell-wall, the extine the cuticular layer.

CHEMISTRY.

Artificial Alizarine.—At the meeting of the Chemical Society on the 17th of March, Mr. Perkin, one of the Secretaries, read a most interesting paper on the above subject. Mr. Perkin is, we believe, now manufacturing this substance on a tolerably large scale, and hence all the more interest attaches to it. After giving a long account of the theoretical methods of reasoning which led Graebe and Liebermann to prepare the colour of madder artificially, Mr. Perkin gave a description of his own method; by means of Mr. Ladd's excellent electric lamp, he showed the colours of the different preparations of the two substances, the natural and the artificial. He then threw the spectra (absorption bands) of the two upon a white screen, and thus exhibited at a glance the identity of the artificial and natural alizarine. Further, he exhibited a number of stuffs dyed with the new compound, and demonstrated that they were equally good as those dyed

with madder. This practical application by Mr. Perkin of a discovery which might otherwise have remained in the laboratory, may be regarded as one of the most important chemical events of late years.

The Constitution of the Ammonium Carbonates.—At the same meeting as that above named, Dr. Edward Diver read an important paper on the relation of the Ammonia Carbonates and Carbamate. But as the further reading of Dr. Diver's paper was postponed to the next meeting of the Society, we must defer an account of it till our July number.

Absorption of mixed Vapours by Charcoal.—Mr. John Hunter of Queen's College, Belfast, who published a paper on the above subject nearly two years ago, again brought his views forward at the meeting of the Chemical Society on January 20th. In experimenting on the absorption of a mixture of two vapours by means of cocoa-nut charcoal, he found that the absorption was increased when one of the vapours was at a temperature near to its point of condensation; and he explained the phenomenon by assuming that, when a fragment of charcoal is introduced into a mixture of two vapours, the one which is nearest to its point of condensation is first absorbed, and this, in its condensed state in the pores of the charcoal, aids the absorption of the other vapour. According to this view, a succession of condensations is going on. The theory is strikingly illustrated in experimenting with a mixture of water vapour and ammonia gas (obtained by heating an aqueous solution of ammonia of sp. gr. 0·88), when the mixture is much more largely absorbed than either the gas or the vapour separately. The mean of a set of experiments made at 100° and a mean pressure of 706·2 m.m. was 316·6 vols. of the mixture absorbed by 1 vol. of charcoal.

Apparatus for the Analysis of Carbonates.—Dr. C. A. Cameron of Dublin described in a recent number of the *Chemical News* an apparatus for the above purpose which he thinks simpler than any other. His apparatus is a light bottle, of the capacity of 75 centimetres. The lower part is divided into two compartments; in one of which the carbonate is placed, in the other the acid. By inclining the bottle, the acid may be allowed to flow over on the carbonate as gradually as the operator pleases. One or two chloride of calcium tubes are inserted through the cork. The cost of this bottle would be about 6*d.*

Chemist to the Mint.—Mr. W. Chandler Roberts has been appointed to the above office. The post is, we believe, a new one.

Artificial India Rubber.—*Les Mondes* (February 3rd) contains a description of a compound under this name prepared by M. Grainer. This material is a mixture containing gelatine and a variety of other substances (not specified) producing a homogeneous elastic substance, insoluble in mineral, as well as vegetable essential oils; not acted upon, moreover, by either coal or other hydrogenised gases. This material is now employed in France for a variety of purposes, too many to be here enumerated; its cost is only 3 francs per kilo.; and it melts readily at 100°, without decomposition, and can be cast into different moulds. Neither cold nor heat affect this substance, which, when completely oxidised, becomes more infusible than vulcanised caoutchouc. See also *Chemical News.*

Chemistry of Fossil Bones.—The *Comptes rendus* for December contains an important paper stating the results of several analyses of fossil bones,

undertaken by M. Schewrez-Kestner. The author points out and proves that chemical analysis can decide the question of the respective ages of bones of various animals found mixed up together, and thus prove that these beings have not necessarily been contemporaneous with each other. The paper contains a large number of tabulated results of analysis made with the special view to careful estimation of the organic matter contained in the various bones. The following are examples of analysis in 100 parts:—Parietal human bone—Ordinary osseine, 3·1; osseine, soluble in dilute HCl, 12·3; water, 6·0; silica, 3·5; bone-earth (phosphate and carbonate of lime), 4·47. Fossil-horse—Ordinary osseine, 3·9; soluble osseine, 3.9; water, 6·8; silica, 0·3; bone-earth, 79·3. Mammoth bones—Ordinary osseine, 2·8; soluble osseine, 8·9; water, 5·7; silica, 12·4; bone-earth, 70·1. These bones were found in the Lehm, near Colmar, Bas-Rhin.

New Reactions of Alcohols.—Mr. Chapman communicated an interesting paper to the *Chemical Society* (Feb. 3rd.) Amylic alcohol, as commonly obtained, consists of two liquids, one rotating a ray of polarised light, the other not. The two may be separated by distilling the mixture from soda calcic chloride, &c. The non-rotating alcohol is retained; the rotating distils over. But, by repeated distillations, it was found that the rotating alcohol is converted into the non-rotating by the very treatment employed to separate the two. No difference in the physical properties of the two alcohols is perceptible. The compounds of the non-rotating liquid do not turn the ray of polarised light; those of the rotating do, and that in an opposite direction to the original alcohol. These facts seem to indicate that the internal structure of organic compounds is not so permanent as we are in the habit of thinking. Another observation Mr. Chapman made whilst pursuing these experiments was that caustic soda is not merely unable to dry alcohol, but that it actually hydrates it. On proper investigation, it turned out that the sodium replaces the hydrogen of the alcohol, whilst the displaced hydrogen takes the place of the sodium in the caustic soda, and thus produces water.

Action of Shellac on Aniline Colours.—A paper published in the *Moniteur Scientifique* of Jan. 15, by M. Labouret, is thus abstracted from the *Chemical News*. When a salt of rosaniline is added to a solution of any resin, that solution is red-coloured, if the salt of aniline is soluble in the solvent used to dissolve the resin; the colour has, however, a tendency to turn violet as soon as the solution is heated or evaporated to dryness. An alcoholic solution of shellac, to which fuchsine has been added, turns, on evaporation, to a most magnificent blue colour. This material is insoluble in ether, but soluble in alcohol and acetic acid, the solutions exhibiting a blue colour. The product is, however, very unstable; and the only use this reaction could be turned to is, according to the author, the detection of shellac among other resins, since a very minute quantity of the last-named resin may by this means be detected.

A Chemical M.P.—In addition to Dr. Lyon Playfair the House of Commons can now count another chemical member in Mr. U. C. J. Kay-Shuttleworth, the representative of Hastings. This gentleman's excellent little work was some time since reviewed in these pages, and we need not say that it shows its author to be an able supporter of modern philosophical doctrines.

Professor Hofmann's Banquet. — Professor Rammelsberg having been elected President of the German Chemical Society for the present year, the Society gave a banquet to the late President, Dr. A. W. Hofmann. The banquet was presided over by Herr Magnus. Among those present were the following: Dr. R. Virchow, Prof. Dove, Prof. Rose, Dr. du Bois Reymond, His Excellency the Right Hon. Mr. Bancroft, the United States Minister at Berlin, and a very large number of *savants* from Berlin and other parts of Germany. Many of the foreign members sent telegrams of felicitation, and among these the following was received from M. J. Dumas, Paris: "Your festive meeting is heartily shared by all chemists of the world who admire and cherish you." Just before the meeting broke up, there was distributed a photo-lithograph, representing Dr. Hofmann under the emblem of Jupiter Ammon (typifying his researches on ammonium), with an aureola of aniline colours. A large number of toasts were heartily responded to; and an ode to aniline, composed for the occasion, caused general applause and great merriment.

Strontia and Ammonia in the Manufacture of Soda.—It is stated by M. Ungerer, in the *Revue hebdomadaire de Chimie* (January 6th), that when to a concentrated solution of sulphate of ammonia is added an equivalent quantity of chloride of sodium, and the fluid heated to boiling-point, mutual decomposition of these salts takes place, and sulphate of soda and chloride of ammonium are formed; the former salt separates as a crystalline powder, and may be removed by filtration from the solution of the chloride of ammonium. The sulphate of soda, having been dissolved in water, may be decomposed by caustic strontia, thus yielding caustic soda; the chloride of ammonium may be converted into carbonate of ammonia by means of chalk. It is doubtful whether this process, theoretically correct, will be commercially available.

GEOLOGY AND PALÆONTOLOGY.

The Chemical and Mineral characters of Lavas.—In a long and able paper on Lavas in the March number of the *Geological Magazine*, Mr. G. Poulett Scrope, in treating of the varying mineral and chemical characters of lavas, says that he cannot but think that far too much importance has been attached to these distinctions, especially by the German geologists. By many of these, as in the instance of Baron von Richtofen, whose classification of Volcanic Rocks was late reviewed by him (*Geol. Mag.*, vol. vi. p 518), these differences, in their minutest peculiarities, have been laid down as determining the relative age of the respective rocks. There can be no greater source of error. It is certain that many varieties of trachyte and basalt, and rocks of intermediate mineral character—that is to say, with a greater or less proportion of acid or basic elements in their composition—are often found succeeding each other as products of the same volcano, in no definite series; sometimes one class, sometimes another, having been first ejected. Nay, they are to be seen occasionally, though rarely, to pass into each other in the same mass, just as some granites are found locally passing into syenite, and this again into greenstone. There are even lavas, as for example that

called Peperino, so much employed in buildings at Naples, in which zones or lenticular blotches of different mineral character alternate throughout the rock, the augitic matter having apparently separated itself from the more feldspathic by a process of segregation during the efflux of the lava. And Mr. Scrope thinks there need be little doubt that what has taken place in this instance on a small scale has frequently occurred on the large one, within the focus of a volcano, during the, perhaps repeated, processes of alternate fusion and re-crystallisation, to which a mass of subterranean lava has been probably exposed, under varying circumstances of temperature and pressure.

Hungarian Fossil Corals.—At the meeting of the Royal Academy of Vienna on the 13th of January Dr. Reuss presented a memoir entitled "Upper Oligocene (*Oberoligocäne*) Corals of Hungary." It contains the description of corals from beds, hitherto held to be eocene, of the tertiary formations of Mogyoros, Tokod, Dorog, Bayóth, in the vicinity of Gran, in Hungary.

A new Wealden Vertebra.—Mr. J. W. Hulke, F.R.S., whose labours as a geologist are almost unceasing, read a paper on this subject at the meeting of the Geological Society on the 9th of February. The specimen described was one which he obtained last autumn at Brook, Isle of Wight, remarkable for its great size, its extremely light structure, and the extraordinary development of the processes connected with the neural arch. It consists of a thin outer shell, inclosing a very open cancellated tissue, having extremely large spaces, comparable with those of Pterosauria, and surpassing those of the cancellous tissue in any of the known larger Dinosaurs. A wedge and notch, similar in principle to the ophidian zygosphene and zygantrum, but differently placed, are superadded to the ordinary articular processes. A broad horizontal platform stretches along the side of the arch from the transverse process to the postzygapophysis. The neural spine is composite; all the outstanding parts are supported and strengthened by thin bony plates. Only a small part of the centrum is preserved, so that the form of this, and in particular of its articular faces, is not determinable. In concluding his account Mr. Hulke noticed certain textural resemblances between the vertebra and a peculiar Streptospondylian vertebra in the British Museum, from the Weald of the south-east of England.

Railway Sections at Sevenoaks.—Railway sections afford such an easy and excellent mode of studying geology, that the paper read by Mr. Caleb Williams, F.G.S., before the Geologists' Association (March 4) deserves attention. The paper is of too great a length for abstract, but is published in full in *Scientific Opinion.* The author described in detail the various deposits passed through in constructing the tunnel (3,451 yards in length), and on the range of hills on which Sevenoaks is situated, and the sections seen in the cuttings to the north of that tunnel. The lowest beds traversed by these works are of fresh-water origin, are only seen in the neighbourhood of Sevenoaks Weald, and consist of grey, blue, and greenish clays, with many layers of flattened bivalve shells or the genera *Unio* and *Cyrena.* Many of the layers are studded with great numbers of *Cyprides,* mostly as casts in the clay. Detached fishscales are dispersed throughout this group of beds, the rhomboidal scales of *Lepidotus* being the most abundant.

Birds of the Middle Tertiary of France.—In a paper presented to the

French Academy on the 14th of March, M. Alphonse Milne-Edwards gave an account of the Birds of various parts of France, and particularly of Bourbonnais during the Middle Tertiary epoch. The abundant remains of these animals is very great, and M. Alphonse Edwards has already discovered more than sixty, amongst which are some which indicate that the climate of France at this period must have been as warm as that of Southern Africa. The principal representatives of the Tertiary ornithological races are the Parroquets, Ibis, Pelican, Secretaries, &c.

Death of Captain Brome.—Those who know, from Mr. Busk's papers, what excellent work was done to the bone-caves of Gibraltar will regret to learn of the death of Captain Fred. Brome, who did so much towards the exploration of these caverns. Captain Brome died on the 4th of March.

Morphology of the Crinoida Cystidea and Blastoidea.—A valuable memoir on the structure of these groups, illustrated by numerous woodcuts, is now appearing in the pages of the *Canadian Naturalist.* It is by Mr. E. Billings, F.G.S., Palæontologist to the Geological Survey of Canada. Its first part extends over some thirty pages, so that we could not possibly abstract it.—Vide *Canadian Naturalist*, vol. iv. No. 3.

The Volcano Fish.—A paper having appeared some time since in a contemporary, from the pen of the Rev. W. W. Spicer, in which the phenomenon of the expulsion of fish from volcanoes was spoken of as strange and astounding, and the idea being conveyed that the fish must have lived "in the line of fire" before being expelled, Mr. Scrope, F.R.S., writes to *Scientific Opinion*, February 23, as follows:—This sensational version of a very simple fact is one only of several which, on the authority of "the great Prussian traveller," have been repeated by compilers of treatises on volcanic phenomena. The simple fact, I conceive, is that the fish in question lived in the open air in crater-lakes, such as are frequently found at the summit of trachytic volcanoes—for the reason that the fine ash, which is usually the last product of their eruptions, and therefore forms the lining of their craters, is very retentive of moisture, and consequently occasions the production of lakes at the bottoms of these hollows. Of course in these lakes the same kind of fish will probably be found as, by Mr. Spicer's own statement, are met with in other lakes at an almost equal elevation on the outer sides of these very volcanoes.

The Ratio of Temperature to Depth.—In the Proceedings of the Royal Society (January 27th), a paper appears by Mr. E. Hull, F.R.S., entitled "Observations on the Temperature of the Strata taken during the Sinking of the Rose Bridge Colliery, Wigan, Lancashire, in 1868–69." This paper is one of great interest and importance, for it establishes very clearly the truth of the general law of a rise of temperature of 1° F. for every 60 feet of descent. Since the experiments on this subject by Hopkins and others, and which are narrated in the books, little or nothing was done in this direction until the proprietor of the Rose Bridge Colliery, Mr. J. Grant Morris, determined to carry down the shafts from the "Cannel" seam to the "Arley" seam of coal, which was known to lie more than 200 yards below it; and consequently in the spring of 1868 preparations were commenced for carrying out this project. In the incredibly short time of one year and two months the Arley coal was struck,

and was found to be of good thickness and quality. The total depth reached was 808 yards, and the ultimate temperature in the coal itself was found to be 93½° F. The manager of the colliery, Mr. Bryham, sensible of the value of observations on the temperature of the strata at such unusual depths (this being probably the deepest colliery in the world, certainly in Britain), made a series of observations with as much care as the circumstances would admit, and has entrusted them to me for publication. The mode of taking the observations was as follows:—On a favourable stratum, such as shale, or even coal, having been reached, a hole was drilled with water in the solid strata to a depth of one yard from the bottom of the pit. A thermometer was then inserted, the hole having been sealed and made air-tight with clay. At the expiration of half an hour the thermometer was taken up and the reading noted. The paper then gives a series of important facts showing the comparisons between the temperature of the strata and that of the adjacent air.

The Palæontographical Societies' Monographs.—The twenty-third volume of these excellent publications (that for 1869) has been issued. This volume appears in good time, owing to the energy of the Honorary Secretary, the Rev. T. Wiltshire. The contents are as follows:—1. Supplement to the Fossil Corals. Part II. No. 2. (Cretaceous Corals.) By Dr. P. Martin Duncan, F.R.S., &c. (Six Plates, pp. 20.) 2. The Cretaceous Echinodermata. Vol. I. Part 3. By Dr. T. Wright, F.R.S.E., F.G.S. (Ten Plates, pp. 13.) 3. The Belemnitidæ. Part V. Oxford Clay Belemnites, &c. By Professor Phillips, M.A., F.R.S, &c. (Nine Plates, pp. 19.) 4. The Fishes of the Old Red Sandstone. By Messrs. J. Powrie and E. Ray Lankester. Part I., *concluded.* The Cephalaspidæ. By E. Ray Lankester, B.A. (Nine Plates, pp. 29.) 5. The Reptilia of the Liassic Formations. Part II. By Professor Owen, F.R.S. (Four Plates, pp. 40.) 6. The Crag Cetacea. No. 1 (*Ziphius*). By Professor Owen, F.R.S. (Five Plates, pp. 40.)

Lady Geologists.—The *Geological Magazine* for March contains two good papers by Lady Geologists. The labours of rational women of this class are worth more than all the thousand-and-one howls of what the *Saturday Review* justly styles "The Shrieking Sisterhood."

Plants in the Schistose Rocks of the Highlands.—At the meeting of the Geological Society of Glasgow, on January 13, Mr. James Thomson read a paper "On further Evidences of the Existence of Plant-remains in the Schistose Rocks of the West Highlands." Mr. Thomson referred to a paper which he had read at the previous meeting, in which he had mentioned the probability of the occurrence of plant-remains in other parts of the West Highlands. This he had now been able to confirm by having found fragments of wood in a conglomerate, composed of talcose schist, with rounded pebbles of quartz, in Glencoe, where it occurs intercalated between beds of schist, which overlie the granite of the district. In connection with the subject, Mr. Thomson also read a paper "On the Vitrified Forts of Dunskaig and Carradale in Kintyre," He described the form of these forts, and drew attention to fragments of charred wood which he had found imbedded between the angular pieces of the schistose rocks of which they are built. The walls of each of them are about 6 ft. thick, and on examination he had

found that they were only vitrified half through. It was evident, therefore, that the forts had been vitrified by means of the wood found imbedded among the stones, and that it had also been applied along the inner boundary only.

Absence of Glacial Action in Alaska.—Mr. Dall, who has been recently exploring the Alaska district, states that no traces of Glacial action are anywhere to be found. This opens up several important problems.

The Structure of Rocks.—The Royal Irish Academy has voted the sum of 25*l.* to Professor William King, for the purpose of making investigation into the jointing cleavage and foliation of rocks.

Mr. Ruskin on Agates.—Mr. Ruskin continues his curious papers on Agates in the *Geological Magazine.* Some of the plates—which, by the way, are the best part of the communication—are very beautifully executed.

Depth of the Carboniferous Sea.—It would seem from a paper by Mr. Somervail, in the *Proceedings of the Geological Society of Edinburgh,* that the minimum depth of this sea was 50 fathoms and the maximum 1,000.

Mathematical Geology.—The Rev. O. Fisher recently published a very mathematical paper on the elevation of mountains by lateral pressure. The paper is ably criticised by Mr. David Forbes, F.R.S. (*Geological Magazine,* January), who thus sums up his opinion of it:—"Mr. Fisher's paper is a purely mathematical one, and altogether beyond the scope of my present communication; yet what I have already stated is but another proof of the tendency which mathematicians apparently have to treat experimental data in their own way, overlooking the vital importance of thoroughly sifting their evidence, or premises on which they base their elaborate reasonings. The recent notorious case of the Pascal forgeries is but an instance in point; and a representative of the Press pleasantly remarked that nobody but a mathematician could have been deceived by such imperfect evidence."

Slates. Felsites—Elvanites.—In the *Philosophical Magazine* for January Mr. J. S. Phillips points out the use of the microscope in investigating the structure of rocks. He describes certain of the above-named rocks from Knockmahon, Co. Waterford, and he also gives the analysis of them. He supplies, too, an account of the microscopic construction. We quote his account of their sections:—

"*Felsite,* spec. grav.=2·64.—Under the microscope this was found to consist of a colourless and generally amorphous matrix enclosing a few dodecahedral crystals of quartz and some small crystals of felspar. Other portions of the matrix appear to be indistinctly crystalline, and to enclose a few laminæ of a greenish mineral, probably chlorite.

"*Elvanite,* spec. grav.=2·66.—Examined under a ½-inch objective, this rock is seen to be composed of an amorphous greyish matrix, in which are porphyritically imbedded crystals of quartz and felspar, the latter being chiefly oligoclase. In addition to these, a few small crystals of some hornblendic mineral are sparingly disseminated throughout the mass. It was further observed that the larger quartz crystals are sometimes penetrated by crystals both of felspar and hornblende; and when examined under a high power, the quartz is seen to contain fluid-cavities.

"*Metamorphosed Slate?,* spec. grav.=2·65.—A section was found under the microscope to be chiefly composed of felspathic-looking crystals crossing

each other in all directions, with here and there some minute scales of chlorite. These crystals, which readily depolarise polarised light, are nearly transparent; but the small amount of potassa, soda, and lime present in the rock, as shown by analysis, renders it improbable that so large a proportion of it can consist of any variety of felspar."

Professorship of Geology in King's College.—The chair has been given to Dr. P. Martin Duncan, F.R.S., Honorary Secretary to the Geological Society of London, and so well known for his researches on Fossil Corals.

MECHANICAL SCIENCE.

Farey and Donkin's Steam Engine.—The injurious effect of water in the cylinder of steam engines on their efficiency, is now pretty well understood. Water may exist in the cylinder, either as priming water carried over from the boiler, or as the result of condensation during the expansion, or from condensation on the surfaces of the cylinder, cooled during the expansion and exhaust, at the moment of admission of fresh steam. The two former causes of loss are best obviated by superheating the steam, the last by a steam jacket, which also tends, though less perfectly than a superheater, to prevent condensation during expansion. In all the best expansive engines now constructed, these means of increasing the efficiency of the engine and reducing the consumption of coal are applied. But in the use of the steam jacket hitherto, while the sides and generally the ends of the cylinder have been heated, the piston has been left undefended against the cooling influence of the expanded steam. It has been proposed in France to convey steam to the interior of the piston, to maintain its temperature in the same way as the temperature of the cylinder sides is maintained by means of the jacket, but whether the proposal has been actually carried out in France we do not know. It has recently been accomplished in this country by Messrs. Bryan, Donkin, & Co. Further, in compound engines Messrs. Donkin have applied a steam superheater between the high pressure and low pressure cylinder. The steam lowered in temperature by a certain amount of expansion in the high-pressure cylinder is again raised to its first state by passing between plates heated by steam from the boiler, before entering the low-pressure cylinder. This mode of superheating presents certain practical advantages, and is said to have effected an economy of 7½ per cent.

Mont Cenis Railway.—Engines have been constructed for the Mont Cenis Railway, with two outside cylinders driving the vertical wheels, and two inside cylinders driving the horizontal wheels which grip the mid rail. This system has been adopted to obviate the difficulties which attend the arrangement of toothed wheels first adopted, and the complicated linkwork subsequently adopted to drive the four pairs of wheels of the mid-rail system, by means of a single pair of cylinders. The working weight of the new engines is 24½ tons. In regard to the Mont Cenis tunnel, 9,225 mètres were completed last year, and another year and a half is expected to suffice for its completion.

Armour-plated Ships.—In a recent letter to *Engineering*, Mr. John Ericsson,

the designer of most of the American Monitors, after defending the Monitor system, states his conviction that impregnability is no longer attainable by the application of a belt of armour reaching a few feet below the water-line. "Up to the present time," he says, "Mr. Reed (the Chief Constructor of the English Navy) has unquestionably distanced all his competitors; the 'Thunderer' and 'Devastation' are in fact impregnable *above* water. But it will soon be found that invulnerability calls for devices wholly different from those which are now deemed sufficient. Much has, however, been gained by the British Admiralty carrying out Mr. Reed's adaptation of the Monitor system. The problem has thereby been greatly simplified, and all further speculation as to what ought to be done *above water* may now be regarded as waste of time. The field has been most materially reduced; our labours being now confined to devising means for piercing the hull below the point that terminates the protection for which Mr. Reed is indebted to the skill of Sir John Brown."

Longitudinal Framing of Ships.—Mr. E. J. Reed, the Chief Constructor of the Navy, has recently called attention to the loss sustained by merchants from the persistence of shipbuilders in adhering to the transverse system of framing in ships. On the longitudinal system, introduced originally by Mr. Scott Russell, and adopted with modifications by the Admiralty, a given strength can be obtained with considerably less weight of hull.

Armour-plate Fastenings.—In combination with the system of raised screw threads for armour-plate bolts, Major Palliser and Lieut. English have introduced a form of nut with spherical bearing surfaces. The spherical nut fits in a spherical recess formed in the armour-plate, or on a washer with a spherical seat. These bolts and nuts can adjust themselves to the twisting of the plate produced by the impact of shot, and are less liable to be wrenched off. In conjunction with a plan of backing armour-plates for land defences with "iron concrete," they are being tried at Shoeburyness.

Whitworth and Palliser Shot.—Major Palliser has challenged Mr. Whitworth to a competitive trial of the relative penetrative power of chilled, pointed, and steel flat-fronted shot.

Atlantic Railway.—In his recent presidential address to the Institute of Civil Engineers, Mr Vignoles, C.E, F R.S., alluded to the Atlantic Railway and the Suez Canal. In regard to the former, he stated that he had received authentic intelligence that on the day previous to the formal opening of the line, ten miles were laid by one working party and eight miles by another. An official inspection, since the opening, has been made, and the conclusion has been arrived at that it will require a further expenditure of some two millions sterling to perfect the way and to comply with the conditions of the contract. These requirements have not been thought unreasonable, and the money is forthcoming for their execution. Meanwhile, the traffic throughout is continued without interruption.—In regard to the Suez Canal, he stated that between May 15 and June 15 of last year, 58 steam dredgers and 11,000 labourers were at work, and upwards of 2½ millions of cubic yards of material were excavated, being at the rate of 80,000 cubic yards daily. He expected that in the coming spring a depth of 26 feet of water throughout will be attained. He believed that it would prove a very excellent financial speculation.

MEDICAL SCIENCE.

Skulls and Phrenology.—The Anatomical Museum of Naples has received from Professor Miraglia a donation consisting of the skulls of ten criminals, a few of which had remained during more than sixty years stuck up on the walls of Castel Capuano, a monument of the barbarous ages. The Professor has added to this gift an authentic history of the skulls, accompanied by various physiological and anatomical considerations on their appearance; and he purposes to adopt this as the subject-matter of a course of lectures which he will shortly deliver at Naples.

Is there Sensation in the Head of a decapitated Man?—The *Lancet* recently gave an interesting account of some experiments made on the head of a culprit who had been guillotined subsequently to the Troppmann affair. The head of the culprit was delivered to the experimenters five minutes after the execution, and was subjected to various processes which are most curious, and imply no small courage on the part of the investigators. First, MM. Evrard and Beaumetz tell us that the head was placed on a table covered with compresses, so as to show the amount of blood which would be obtained. The face was then bloodless, of a pale and uniform hue; the lower jaw had fallen and the mouth was gaping. The features, which were immovable, bore an expression of stupor but not of pain. The eyes were open, fixed, looking straight before them; the pupils were dilated; the cornea had already commenced to lose its lustre and transparency. Some sawdust still stuck here and there to the face, but there was no vestige of any either on the inner surface of the lips or on the tongue. This is an important fact. The opening of the ear was then carefully cleansed, and the experimenters applying their lips as closely as possible to the orifice called out three times in a loud voice the name of the criminal. Not a feature moved; there was no muscular movement, either of the eyes or on the face. A piece of charpie, saturated with ammonia, was next placed under the nostrils; there was no contraction of the alæ nor of the face. The conjunctiva of each eye was deeply and several times successively cauterised with nitrate of silver; the light of a candle was brought within two centimètres distance of the cornea, and yet no contraction was observed either in the eyelids, eyeball, or the pupils.

The Muscular Power of the Uterus estimated Mechanically.—At a meeting of the Royal Society on March 10, the Rev. Professor Haughton read a paper on the mechanics of labour. Dr. Haughton, as usual, went thoroughly into his subject and produced a most elaborate and interesting paper. He states that, in the first stage of natural labour, the involuntary muscles of the uterus contract upon the fluid contents of this organ, and possess sufficient force to dilate the mouth of the womb, and generally to rupture the membranes. He endeavours to show, from the principles of muscular action already laid down, that the uterine muscles are sufficient, and not much more than sufficient, to complete the first stage of labour, and that they do not possess an amount of force adequate to rupture, in any case, the uterine wall itself. In the second stage of labour, the irritation of the fœtal head upon the wall of the vagina provokes the reflex action of the voluntary abdominal muscles, which aid powerfully the uterine muscles to complete

the second stage by expelling the fœtus. The amount of available additional force given out by the abdominal muscles admits of calculation, and is found much greater than the force produced by the involuntary contractions of the womb itself.

The antagonistic Action of Strychnine and Chloral.—Now that chloral is coming into general use as an hypnotic it is interesting and important to note that M. Liebreich has discovered the fact that strychnia is antidotal to chloral. The results of M. Liebreich's novel researches were laid before the French Academy of Sciences a few weeks ago by Professor Wurtz, of the Paris Faculty. M. Wurtz stated that the attention of the Berlin experimenter had been drawn to strychnia by the observation of a case of trismus, which after having persisted eight days was immediately cured on the employment of chloral. This led him to produce, artificially, the phenomena of tetanus in animals by the administration of strychnia, in order to study the effects of chloral on this artificial condition. He then observed that chloral diminished the effects of strychnia, provided it were exhibited very soon after the administration of the alkaloid. Another important result was obtained when M. Liebreich, pursuing his researches, discovered the effect of strychnine on animals poisoned by large doses of chloral. He then made out that strychnia, administered after a too heavy dose of chloral, shortens and destroys the effects of this substance, yet without producing the injurious action which it exhibits in ordinary cases. Therefore M. Liebreich proposes to employ injections of nitrate of strychnia as an antidote in cases where accidents have occurred through an overdose of chloral or chloroform.

An Italian surgical Prize.—The Medico-Chirurgical Society of Bologna has awarded the Sgarzi Gaiani prize of 2,000 lire (80*l.*) to Professor A. Corradi, of Pavia, for the best work on the improvements effected in surgery by Italians during the present century.

The Passage of white Corpuscles through the Capillaries.—M. Feltz contradicts the well-known recent experiments which seem to show that the white blood-corpuscles traverse the walls of the capillaries. He attempts to demonstrate the absence of apertures of the capillaries. We should like to know by what delicate method he effects such a demonstration, for even in the case of the connective tissue, the question as to whether there are or are not minute spaces is one of the most difficult points to determine in the whole range of histology. The question of the apertures in the capillaries is not less difficult.

The four Apertures of the Heart.—Dr. Herbert Davies has published before the Royal Society a long and very valuable paper on the "law which regulates the magnitude of the areas of the four orifices of the heart." The author endeavoured to show that the size of the aperture has an exact relation—according to dynamical laws—to the velocity of the current passing through it.—Vide Paper read before the Royal Society March 17.

A Collection of Greek Skulls for the Hunterian Society.—The Council of the Hunterian Museum are in treaty with Dr. Nicolucci of Isola di Sooa for the purchase of his fine collection of Italian and Greek skulls. This collection, comprising 165 specimens of ancient and modern crania, selected with great

care by the celebrated Italian ethnologist, and upon which his well-known researches into the history of the races of southern Europe have been mainly founded, will prove a valuable acquisition to the already extensive series in the Hunterian Museum.

The Atmospheric Germ Theory.—The atmospheric germ theory has not received much support by the recent researches of Dr. A. Ransom, who read a paper on the subject before the Literary and Philosophical Society of Manchester, at a meeting on February 22. The author gave the following summary of the results of his experiments:—1. In 1857, glass plates covered with glycerine had been exposed in different places and examined microscopically. Amongst others, in the dome of the Borough Gaol, to which all the respired air in the building is conducted, organised particles from the lungs and various fibres were found in this air. 2. During a crowded meeting at the Free Trade Hall air from one of the boxes was drawn for two hours through distilled water, and the sediment examined after thirty-six hours. The following objects were noted:—Fibres, separate cellules, nucleated cells surrounded by granular matter, numerous epithelial scales from the lungs and skin. 3. The dust from the top of one of the pillars was also examined, and in addition to other objects, the same epithelial scales were detected. 4. Several of the specimens of fluid from the lungs were also searched with the microscope. In all of them epithelium in different stages of deterioration was abundantly present, but very few spores were found in any fresh specimen. On the other hand, after the fluid had been kept for a few hours, myriads of vibriones and many spores were found. In a case of diphtheria, confervoid filaments were noticed, and in two other cases, one of measles and one of whooping cough, abundant specimens of a small-celled torula were found, and these were seen to increase in numbers for two days, after which they ceased to develop. These differences in the nature of the bodies met with probably show some difference in the nature of the fluid given off; but it was pointed out that they afford no proof as yet of the germ theory of disease. They simply show the readiness with which aqueous vapour of the breath supports fermentation, and the dangers of bad ventilation, especially in hospitals.

Hygienic Influence of the Air of Hospitals.—At the meeting of the French Academy, March 14, M. Dumas presented a paper in which the author made some remarks on the danger, in a hygienic point of view, which results from the miasmata derived from the air of hospitals. The author called attention to the advisability of purifying the air expelled from hospital wards, either by means of heat or by some other process. In connection with the subject, M. Henri Saint-Claire Deville remarked on this subject that, from experiments made by M. Pasteur upon air expelled from hospitals during the last cholera epidemic, it was observed that some portions of this air were acid and some were alkaline, and that the presence of all kinds of spores had been remarked in it. M. Saint-Claire Deville thinks it would be advantageous to burn this air before it be expelled into the atmosphere.

Inoculation of Pulmonary Tubercle.—At the meeting of the Royal Institution of Lombardy, on Jan. 27, Dr. Biffi then brought forward the results of some recent researches on inoculation of pulmonary tubercle which he had

made in conjunction with Dr. Verga. This paper gave rise to some observations from Dr. Bizzozero, tending to show the apparent discord of results obtained in this matter by many experimenters; this opinion was confirmed in a few words by Dr. Verga.

Death of M. Poisseuille.—We regret to have to record since our last issue the death of M. Poisseuille, the celebrated inventor of the Hæmadynamometer. He had reached the age of 73 years.

Faradization in Physiognomy.—M. Duchenne, so well known for his advocacy of the induction coil in the treatment of nervous diseases, has been applying the art to the study of physiognomy. He has been trying to study the effects of the contraction of the facial muscles on the expression, and in order to prove this he has adopted the plan of "Faradizing" the different muscles, and then photographing the face while the muscle is in contraction. His description of his results is reviewed in the current number of the American *Psychological Journal*, and several capital photographs from Duchenne's negatives accompany the review.

The Physiology of the Iris.—The question as to whether the iris is provided with a special set of dilator and contractor muscles is among the most difficult in physiology. The subject has recently been taken up by various continental physiologists, and among these by M. Dogiel, who has written a paper in a recent number of *Max Schultze's Archiv für mikroskopische Anatomie.* This observer states he has demonstrated microscopically the presence of both a dilator and a sphincter in a considerable number of animals with the greatest certainty. These being admitted, then, the next point is to determine in what way the movements of the iris are influenced by the third nerve and the sympathetic respectively. To determine this it seemed to be of great importance to discover how the iris behaves when both sets of fibres are either directly or indirectly through these nerves stimulated to the utmost extent; and this may be effected either by the application of two oppositely-charged electrodes to the fibres, or by acting on one set of the fibres with one electrode and the nerve supplying the opposite set of fibres with the other electrode, or by stimulating both nerves coincidently. When both sets of fibres were directly stimulated, the pupil retained its average diameter and circular form, or occasionally became elliptical. When one set of muscles was directly stimulated and the other excited through the nerves, no remarkable preponderance of one over the other set was observed; but when both nerves were simultaneously excited the action of the third was decidedly strongest, strong contraction of the pupil resulting, and it was noticed that the action of the stimulus, when applied through the nerves, was in all instances much greater than when applied directly.

MINERALOGY AND METALLURGY.

Crystals in Iron as seen with the Microscope.—Some recent microscopical examinations of iron and steel have been made by M. Schott, and have led him to believe that all crystals of iron are of the form of a double pyramid, the axis of which is variable, as compared with the size of the base. The

crystals of the coarser kinds, as compared with those of the finest qualities of crystalline iron, are of about twice the height. The more uniform the grain, the smaller the crystals, and the flatter the pyramids which form each single element, the better is the quality, the greater is the cohesive force, and the finer the surface of the iron. These pyramids become flatter as the proportion of carbon contained in the steel decreases. Consequently in cast iron and in the crudest kinds of hard steel the crystals approach more the cubical form, from which the octahedron proper is derived, and the opposite extreme or wrought iron has its pyramids flattened down to parallel surfaces or leaves, which in their arrangement produce what is called the fibre of the iron. The highest quality of steel has all its crystals in parallel positions, each crystal filling the interstices formed by the angular sides of its neighbours. The crystals stand with their axes in the direction of the pressure or percussive force exerted upon them in working.

The Commercial Preparation of White Lead.—We learn from the *Chemical News* that an invention is at present being tested by which ordinary galena, after having been crushed in an ore crusher, is roasted in a desulphurising kiln, and next mixed with carbon (preferably in the state of fine-washed pea or dust anthracite coal) in the proportion of half and half. This mixture is heated in a peculiarly constructed furnace; and the dense white vapours which are given off are conveyed into a separate chamber, and strained by passing through bags or screens of muslin, or are allowed to deposit slowly, and, after cooling, collected, as in the case of the manufacture of zinc-white.

Gold in the Laurentian Rocks of Nova Scotia.—Professor Hind has lately made a report on the rocks which, according to him, represent the Laurentian system of Sir W. Logan. The *Mining Journal* states that Professor Hind considers these rocks to consist of sedimentary deposits, altered by metamorphic action, and that their crystalline structure was produced before the deposition of the gold-bearing rocks which lie unconformably upon them. Gold is found and worked in the Laurentian rocks in Canada, as at the Madoc Mines, and it is not improbable that mines may yet be opened in the Laurentian of Nova Scotia. The area occupied by the Laurentian in Nova Scotia is probably not less than one-half that part of the country which has hitherto been represented on geological maps as Lower Silurian. The Laurentian, being auriferous inferentially, may yet turn out to be as valuable in mineral wealth as the Silurian deposits now worked. In Canada, there are, besides gold, beds of magnetic iron ore, of sulphurets of iron and copper, and of titaniferous iron ore in these rocks, which may also be found in the Laurentian of Nova Scotia, and being in close proximity to the coal-fields, may yet prove a great source of wealth.

A new Explosive for Mines.—A substance which has been called Dualin is now coming pretty extensively into use among American miners. It is a yellowish-brown powder, resembling in appearance Virginia smoking tobacco. It will, if lighted in the open air, burn without exploding; but, if confined, it may be made to explode in the same manner as common powder. It is not sensitive to concussion; will not decompose by itself, nor cake or pack together; may be readily filled into cartridges; and it matters not whether the place where it is stored be warm or cold, dry or damp. It has

from four to ten times the strength of common powder, and is stronger than dynamite.

The Preparation of Artificial Porphyry.—It is stated that MM. Sepulchre and Ohresser have perfectly succeeded in utilising the slag of the iron blast furnaces for the manufacture of paving stones, which withstand a crushing weight of more than 400 kilos per square centimètre, and have answered for the purpose of paving several streets at Brussels and Paris, and stood heavy traffic far better than even the celebrated Quenast stones. The streets paved with this material at Brussels have a heavy gradient.

Graphite in Ceylon.—According to the statements lately made in some of the Ceylon papers, it would seem that there is a large abundance of plumbago in that island. The following quotation is what we refer to:—Fresh discoveries of the mineral are constantly made. Should mining continue at the rate of the past few years, government will have to regulate the pursuit with reference to the safety of the people, otherwise lives will be lost from foul air and the collapse of badly-formed pits. We observe that the Chamber of Commerce found the specimens of this mineral sent some time ago from Hambantotte to be defective from the presence of "rust," or, as the natives call it, "water mark." The progress of this staple export has been from 46,000 cwt. to, in round numbers, 200,000. The quantity has considerably more than quadrupled in five years, and more than doubled in the past as compared with the previous season.

The Recently Discovered Coal in India.—The *Mining Journal* says that the question as to the precise character of the mineral discovered in the Kistnah district is at present attracting considerable attention, and it has received from Colonel F. Applegath, of Vizanagram (who, it will be remembered, found and burnt coal at Jaggiapetta some years ago), a small specimen of the fuel, which may be examined at its office by those interested. The specimen forwarded is too small to permit of a decided opinion being formed respecting it, but Dr. Benjamin H. Paul, F.C.S., to whom the editor has submitted it, and who has long devoted special attention to the examination of fuels, states that it appears to possess all the characteristics of a lignite or brown coal. From the fact mentioned, that the sample is taken from near the outcrop, he considers it would not be possible to judge with certainty of the quality of the mineral as fuel, though it is probable it would prove very valuable in this respect, especially in India.

Discovery of an Ancient Silver Mine.—The recent earthquakes in Germany have caused the fall of a large mass of rocks situated between Heidelberg and Wiesloch, and in consequence thereof the works of a silver mine, worked by the ancient Romans, have been brought to light. There is no silver-ore of any importance left, but, instead, a very rich zinc ore is met with in large quantity, which was left untouched by the former workers.

The Examination of Alloys.—In a paper published in the *American Journal of Science* for January, Mr. F. W. Clarke gives an account of a simple mode of examining an alloy. A couple of years ago he made a few experiments upon indirectly determining the proportions of tin and antimony in alloys of the two metals. He oxidised a weighed quantity of the alloy with nitric acid in a porcelain crucible, heated the resulting oxides with ammonic nitrate, and then (regarding the tin as converted into SnO_2, and the anti-

mony into Sb_2O_4) calculated the proportions of the metals from the increase in weight. This method, although by no means giving him accurate results, served very well for rough approximate determinations. He cites it simply as an easy and convenient process for obtaining a close idea of the constitution of any alloy composed of the two metals. Possibly the method might be so modified as to give accurate determinations.

MICROSCOPY.

The Inhabitants of the Mouth and Teeth.—Under this somewhat uninviting title a paper has lately been written by Herr Schrott, and has been translated in the American *Dental Cosmos* by Herr A. Petermann of Munich. We cannot abstract all the writer says, but the following account of the Spirillæ is not without interest. Spirillæ are a variety of the vibriones. They are found in hollow teeth in which the saliva does not change daily. Very often some, with one or two windings, are found between the teeth and under artificial sets. No microscopical object stimulates the admiration of the spectator more than these little lines in the form of a screw, and which move, like living corkscrews, with immense rapidity, forward and backward, without it being possible for the eye or the mind to conceive in what manner these relations are effected. To have a very good view, it is necessary to cut out a little piece of damp caries from a newly-extracted tooth; put this in one or two ounces of distilled water, cover the vessel in such a way that the air has a little entrance, and keep these infusoriæ two or three weeks in this manner. After this time, if you take the caries out of the glass and put an adherent drop of water under the microscope, you will observe an immense quantity of spirillæ, which obtain from one to six windings.

A good Preservative for mounting Animal Tissue.—In a recent number of Brown-Séquard's *Archives of Physiology* it is said that carbazotic acid is a capital mounting substance. This acid is only moderately soluble in water, and a saturated solution may therefore be employed. It possesses the further advantage of being very cheap. It is admirably adapted for all tissues containing much blood, and therefore for specimens of liver, lung, &c. It appears to act by effecting coagulation of the albuminous substances, though, unlike alcohol and chromic acid, it does not occasion any fusion of the constituents of the tissue. The red globules retain their form and characters extremely well. The portion of tissue required to be examined should be plunged into the solution, and after the lapse of twenty-four hours it will be found to have acquired sufficient firmness to permit of very fine sections being made with a razor. The saving of time by this method, as compared with the chromic acid, is immense. The preparations will take colour from carminate of ammonia, and may be preserved in glycerin.

Cheap and good Immersion Objectives.—The *Monthly Microscopical Journal*, quoting Max Schultze, states that a German optician, Gundlach of Berlin, has succeeded in making better and cheaper object-glasses than even Nachet or Hartnack. He sells an immersion $\frac{1}{12}$-inch at as low a price as 2*l.* 12*s.* In this country Mr. Crouch is in advance of the English makers in offering an

excellent $\frac{1}{8}$ on the immersion plan for the sum of 4*l.* or less, according to the presence or absence of an adjusting arrangement.

The Microscope in the Pyx Chamber.—The mysterious chamber known as the Pyx at Westminster, and in which the standards of measure are now kept, has nailed on its door a piece of skin said to be human. This has been examined by Mr. H. F. Hailes, who read a paper on it some time ago at the Quekett Club. Mr. Hailes thinks the specimen human skin, but does not appear to be quite certain.—*Journal of the Quekett Club.* (January.)

The Microscope in the Welsh Fasting Girl Case.—In the course of the recent prosecution in connection with this case, it came out that Mr. John Phillips had examined with a microscope the contents of the stomach. He recognised starch globules in abundance, and several small pieces of bone—either of small fish or small birds. The starch was most probably taken from arrowroot.

Polarising Objects from the Hydrocarbons.—At a recent meeting of the Literary and Philosophical Society of Manchester, Mr. J. B. Dancer, F.R.A.S., read a short paper on some of the new hydrocarbon compounds from which he had obtained very beautiful polarising objects for the microscope. These were exhibited to the members, and a more detailed account promised when the experiments are complete.

Movement of Chlorophyll Corpuscles.—At the meeting of the French Academy on January 17, M. Brongniart presented a note from M. Rose, relative to M. Prillieux's recent researches on the influence of light on the movement of the chlorophyll corpuscles. The motion of the chlorophyll corpuscles is not, he says, produced separately in each individual corpuscle. On the contrary, the light affects a mass of matter which incloses a number of corpuscles and moves them bodily along with it.

Illuminating opaque Objects under high Powers.—Mr. Tolles, the well-known American maker, has produced an improvement on Professor Smith's well-known combination. It is thus described in the *Monthly Microscopical Journal*:—In this a prism is inserted in the side of the objective, between the front and middle combinations, of such a shape that a beam of light, received at the side of the objective, is thrown by a totally reflecting surface through one side of the front lens, at such an angle that none of it is reflected, but all passes through and is condensed on the object, and from that reflected back to the eye. Only one of these instruments (now owned by a physician of Boston) was then made. Recently Mr. Tolles has made two more of them, and their performance is such as to promise that little, if any, improvement can be expected in this direction. Opaque objects are seen with $\frac{4}{10}$-inch and quarter-inch objectives (from 200 to 500 diameters), brilliantly illuminated on a black background. The appearance of diatoms is similar to that obtained with the parabola, but the details of surface are shown with a distinctness never before seen. Of how much utility this is to prove, and what discoveries are to be made in the works of nature with it, are among the problems that the microscopists are called on to solve.

Dust under the Microscope.—Dr. Sigerson, of the Catholic University, Ireland, has been recently making some experiments in this direction. The results were lately laid before the *Royal Dublin Society.*

A Scale for the Microspectroscope.—The February number of the *Monthly Microscopical Journal* describes a plan of estimating the position of absorption bands which is likely to supersede all others hitherto employed. The plan consists in throwing, by lateral illumination, a bright line (a beam from a lamp, sent through a line photographed on a glass plate, the rays being rendered parallel by a lens) upon the spectrum. By means of a micrometer screw the line may be made to travel over the whole of the spectrum, and thus the exact position may be read off in numbers of the screw.

Nature of the so-called Brownian Movements of Microscopic Particles.—Professor W. Stanley Jevons read an interesting paper on this subject before the Philosophical Society of Manchester on January 25. The paper is too long for abstract, but we may quote the following passage as indicating the author's views:—I consider it to be established experimentally that the microscopic movement is due to electric action, and if I may venture to suggest a somewhat speculative explanation of the action, I would point to the experiments of M. Wiedemann on electric osmose. It was first observed by Mr. Porret that when the poles of a battery are placed in two portions of water separated by a porous division, not only is some of the water decomposed, but another and far larger portion is impelled towards the negative pole. M. Wiedemann having exactly investigated the phenomenon, found that for one part of water decomposed 5,000 parts were transported through the porous septum. This impulsion is greater as the resistance of the liquid is greater, and ceases altogether when sufficient acid or salt is added to render it a good conductor. Every particle which is thrown into a polar condition by the action of water must be capable in a minute degree of exerting a similar force. In ordinary osmose the particles being fixed cause a transportation of the fluid; in microscopic movement, on the other hand, the particle is free to move, and the reaction of the liquid probably produces those movements which are visible in the microscope.

PHOTOGRAPHY.

The Possibility of obtaining Heliochromes.—Mr. Glaisher, the President of the Photographic Society, having at a recent meeting of that body stated it to be his opinion that the laws of light would require to be changed before photographs in their natural colours could be obtained, has had this dictum controverted by several authorities on this subject. Among others, the Rev. J. B. Reade says that it is because the laws of light are what they are that they have the best chance of succeeding in obtaining heliochromes. The editor of a photographic contemporary considers that it is somewhat singular that the President of the Photographic Society should be unacquainted with the fact that, even without any change in the laws of light, heliochromy is an accomplished fact. The difficulties attending upon their production and the sombreness of their hues interpose obstacles to their being generally introduced, but the scientific possibility of their production has been both proved and demonstrated. He describes the method by which was obtained a photograph (in colours) of the solar spectrum that

was made by M. Becquerel for the late Sir David Brewster:—A polished daguerreotype plate having been connected with the positive pole of a Grove's battery, and a piece of platina foil having also been connected with the negative pole, these are immersed in a solution composed of one part of hydrochloric acid in eight parts of water. After a brief immersion, the silver surface of the plate becomes first violet and then black from the formation of subchloride. In this state the plate, after being gently rubbed and washed, is ready to receive the natural colours. It has been suggested that papers prepared with the sub-chromate of silver will be more likely to yield satisfactory results in heliochromy than those which owe their sensitiveness to the subchloride.

A Poor Man's Photography.—Under this apparently whimsical title, Professor Piazzi Smyth, the Astronomer Royal for Scotland, has published a lecture, delivered by him before the Edinburgh Photographic Society. The object of the lecture is to show how a poor man (the Professor himself) went to the Pyramids of Egypt, armed with a small pocket camera, by which he obtained a number of negatives, only an inch square, but so sharp as to bear considerably enlarging, and how these minute pictures were subsequently more valuable as scientific data connected with the great Pyramid than those taken by the ordnance survey, four years afterwards, under circumstances possessing peculiar facilities for carrying out such an undertaking. Several of Professor Smyth's original negatives of the Pyramid have been exhibited at the London Photographic Society, and have elicited much admiration on account of their great sharpness, clearness, and delicacy. It may be interesting here to observe, that untouched reproductions from these negatives of one inch square have been exhibited by means of the magic lantern, on a scale of sixteen feet.

Photo-engraving.—The details of another process for making engraved plates by means of photography have recently been published by Mr. R. H. Courtenay. While it is in its main features similar to that of Paul Pretsch, it differs in some of its details, and in these points of difference, according to the inventor, are to be found its superiority to any other published process of a similar nature. Its leading features are these:—A plate of glass having received a sensitive coating composed of gelatine and an alkaline bichromate, is exposed to light under a negative that differs from one of the ordinary kind in having finely-ground silica interspersed throughout it. These atoms give the requisite granular condition for holding ink in the copperplate subsequently obtained. After exposure to light the gelatinised glass is immersed in water, by which the surface previously plane now starts into a series of depressions and ridges—it swells out or remains depressed precisely in accordance with the action of the light. This surface, when partially dried, is rendered conducting by means of silver bronze powder or analogous means, and is then used for the purpose of producing an electrotype cast capable of yielding impressions, as in the case of an ordinary engraved copperplate. Mr. Woodbury is also engaged in perfecting a process of photo-engraving which, according to the *Photographic News*, was discovered partly by accident. Observing that in one of his gelatine reliefs some colour which had been added was granulated, he investigated the cause and has since utilised this defect. From a granulated picture in gelatine he obtains an impression

in lead or other soft metal by heavy pressure, and this metallic impression is used as a mould for the production of copperplates by electro-deposition.

The editor of the *British Journal of Photography* has been holding up to ridicule the *Times* fine-art critic, who, in writing about certain photographic productions of Colonel Stuart Wortley, has supposed that because they were intended to resemble *moonlight effects* they were really obtained by the agency of moonlight. He assumes that it would be desirable, if not necessary, that art critics should make themselves acquainted with the principles of the methods by which pictures are produced, by which they will avoid such blunders as those referred to. As the method of producing the so-called moonlight photographs has never been published in the POPULAR SCIENCE REVIEW, it may not be out of place here to publish this supposed secret. In selecting the subject let it be, if at all possible, an aquatic one; no other answers nearly so well. The view must be illuminated by strong sunshine, and, unlike the usual method of taking an ordinary picture, the camera must be directed towards the quarter from which the sun is shining. The exposure must be as nearly instantaneous as possible. If the vertical angle of the picture be such as to include the sun, so much the better; but as the powerful luminousness of the sun will cause such an amount of optical natation as will entirely destroy the pictorial effect, it is necessary that means to prevent this be had recourse to. One of the simplest, and probably the best, is to put a piece of wet red blotting-paper on the back of the glass plate on which the sensitive film reposes. It is necessary that it be in optical contact, and this is secured by the simple expedient of making the paper quite wet before applying it to the back of the glass plate. When such a precaution as this has not been taken, the photograph of the sun presents a very peculiar aspect, being surrounded with concentric rings. When a print is taken from a negative obtained in this manner the effect is precisely that of moonlight.

Photographing Criminals.—The Home Secretary has been sending intimations to the authorities of several of the larger towns requesting them to take photographs of prisoners and furnish copies to the central authorities in London. It is to be presumed that the central album will have suitable divisions for the respective class of occupants, so that thieves, burglars, murderers, forgers, &c., may be retained in their appropriate places.

PHYSICS.

Application of Optics to Chemistry.—At one of the February meetings of the Royal Irish Academy, Professor Jellet communicated an important paper on the above subject. Having made some general preliminary remarks respecting the changes in rotation observed under the polariscope as the result of the action of acids on bases, he proceeded to the consideration of the changes effected by the action of nitric acid on quinia. From his investigations he was able to establish the existence of an acid nitrate, or binitrate, of that base—a salt which has not as yet been isolated. He promised to make a further communication on the combinations of nitric acid with a mixture of quinia and ammonia.

Nairn's Electric Machine.—In a paper read before the Royal Academy of Belgium, but the report of which reached us too late for our last number, M. Pérard, of Liége, read a note on a modification of Nairn's electrical machine, which gives much stronger sparks, and produces much more powerful effects, than can be obtained with the ordinary machine. The modification consists in suspending a Leyden jar from each of the conductors of the machine, and connecting the outer coatings of the two jars by a chain. The permanent and complete neutralisation of the two coatings thus united gives great energy to the condensation. By employing jars 160 millimètres high and 90 in diameter, M. Pérard has succeeded in piercing a plate of glass 6 millimètres thick, although the glass cylinder of the machine was only 50 millimètres in diameter, and the cushion 380 long. In its ordinary state this machine gives a spark which passes between the extremities of the branches across a space of 45 millimètres; but when the jars are placed as described, the spark occurs much less frequently, and is much louder and more vivid, and can be obtained 100 millimètres long, or even 170 when the atmospheric conditions are favourable. This spark has not the continuity of that of Holtz's machine or Ruhmkorff's coil, but it is much more powerful than the first, and may be compared to that of a large coil for amplitude and brilliancy. The detonation of the spark 170 millimètres long is as loud as that of the spontaneous discharge of a battery of six Leyden jars.

The Source of Solar Temperature.—Padre Secchi publishes a paper in *Les Mondes* (February 24) on the means by which the solar temperature is maintained. Of this the following is a summary:—The sun is a globe possessed of an enormously high temperature, undoubtedly reaching many millions of degrees; but our means of estimating that temperature are very imperfect. As to the origin of this high degree of heat, it may have been the result of the force of gravitation which has united the elements of which the central point of the system (viz. of our solar) has been made up; the initial temperature, therefore, the result of mechanical action, will, of necessity, have been far greater than the present temperature of the sun is, which is certainly cooling down. However great this loss of heat may be, it is imperceptible to us, since it is slowly taking place, and partly compensated by chemical actions which take place in the sun, which is, in all probability, in its interior, a mass of strongly-compressed and condensed nebulous matter.

M. Lamy's New Thermometer.—At the meeting of the French Academy, on Februay 21, M. St. Claire-Deville made a few remarks on a new thermometer invented by M. Lamy. Ammoniacal chloride of calcium gives off gas which, in a closed space, possesses a tension which is directly related to the temperature. If, therefore, a meteorologist places on the roof of his house a small metallic box containing sufficient solution of calcium chloride saturated with ammonia, and connects it by a leaden tube with a manometer in his room, he can observe at any moment, without disturbing himself, the external temperature. M. Faye having proposed to apply this instrument to the study of subterranean temperatures, M. Becquerel called attention to the fact that, five years ago, he had had constructed thermo-electric apparatus which perfectly fulfilled M. Faye's desideratum, and

which act independently without being touched, and which indicate the temperature to the twentieth of a degree.

A Rain of Sand.—A curious shower of sand took place in some parts of Italy on February 13 and 14 last, and has been described in the *Comptes rendus* by M. P. Denza. This memoir, says the *Chemical News*, contains the account of a very curious phenomenon—viz. rain in the southern parts of Italy accompanied by a fall of a fine reddish sand, while, in the northern parts of that kingdom, snow fell accompanied by the same substance. The sand has been tested, and found identical with that which is now and then carried by gales of wind from the African desert, not simply into Italy, but even sometimes into Switzerland, where some of it fell, along with snow, at Tscappina (Canton des Grisons). This paper contains many curious facts relating to a phenomenon which is sometimes observed also on the Canary Islands.

Curious Air-cavities in accidentally-formed artificial Crystals.—Some curious facts were brought lately before the Boston Society of Natural History by Mr. Thomas Gaffield, who exhibited some bottle-stoppers the cavities of which contained water. Mr. Gaffield stated that on the morning of September 6, a fire occurred in the glass-cutting establishment of J. M. Cook, in Congress Street. On the next morning he visited the ruins of the fire to search for any specimens exhibiting the devitrification of glass exposed to great and long-continued heat. He found nothing of this kind, but instead, in a pile of melted glass and cinders of wood, discovered some stopples of glass bottles, cracked on the outside and containing water. These stopples were originally made with a cavity containing a partial vacuum, as the air must have been enclosed when hot, and when cooled must have contracted and filled less space than previously. Mr. Gaffield presumed that when the glass stopples were heated red hot by the fire around them, the stream of water from the engines coming in contact with them produced the cracks through which the water rushed in, in sufficient quantity to fill the partial vacuum. The glass was cooled by the water within, and the fire extinguished by the water without, and so the glass contracting to its original size has virtually almost hermetically sealed the imprisoned water. Mr. Gaffield thought that these specimens might throw some light upon the occurrence of crystals with cavities containing liquids, and of mineral goedes lined with crystals.

An Approach caused by Vibration.—At a recent meeting of the Royal Society, Professor Guthrie gave an account of some very curious results of experiments with vibrating bodies. He showed that when a card was held in a certain position near a vibrating tuning-fork, it was attracted towards it. The author concluded that the effect of apparent attraction is due to atmospheric pressure, and that this pressure is due to undulatory dispersion. He suggested that the dispersion of the vibrations which constitute radiant heat may cause bodies to approach, being pushed, not pulled.

A new Thermo-Electric Pile.—A piece of apparatus of the thermo-pile character has recently been constructed by MM. Muré and Clamond, and has been minutely described in some of the physical journals. It consists of small bars of lead, or native sulphuret of lead, and of plates of steel. The bars are 40 mm. long by 8 mm. thick, and the plates of steel are 55

mm. long by 8 mm. broad, and 0·6 mm. thick. In these couples galena is the electro-negative element; iron, the electro-positive. The form of the bars is such, that by placing them side by side they form a ring of twelve couples, of which the interior is formed by the extremities which are to be heated. They are united in tension by means of tin solder. They are isolated from one another by thin mica plates. By placing five of these rings in a vertical column a battery of sixty couples is formed. These rings are isolated and separated by washers of asbestos. The whole is firmly held between two iron rings by means of three bolts. The pile thus forms a hollow cylinder, the interior of which must be heated. The cooling of the junctions, whose temperature should be lower, is caused by radiation into the air. The interior cylinder measures 50 mm. in diameter and about the same in height. The heated surface is about 78 square centimètres. The apparatus is heated by a gas-burner, consisting of a steel cylinder, 56 mm. in diameter, closed above, opened below, and pierced with small orifices. This is placed in the centre of the pile. A tube pierced with holes surrounds this cylinder, and distributes the gas uniformly around it. The gas rises, and arriving at the orifices in the burners, meets the air which is escaping from it because of the draught of the tube of steel that surrounds the apparatus. Each orifice in the burner thus forms a blow-pipe, the jet of which strikes the opposite side. Forty couples have an electro-motive force equal to that of a Bunsen element.

A convenient Electric Lamp which deserves the careful attention of lecturers, whether they be exhibitors of spectra or of micro-photographs, is that of Mr. Ladd. It is certainly one of the neatest and most regular-working lamps we have ever seen. It did its work admirably at the last meeting of the Chemical Society, when Mr. Perkin showed the spectrum of Alizarine.

A Prize for the best System of Ventilation.—The Royal Danish Society of Science, among other prizes, has offered one amounting to 34*l.* for the best essay containing an investigation of the movement of the air in a system of ventilation. The essay may be written in English, French, German, Danish, or Swedish, and must be handed in before October 1870.

The Vibrations of Tense Strings.—At the meeting of the Vienna Academy on January 22, Professor E. Mach sent in a research on the vibrations of stretched cords, carried out in the physical laboratory of the University of Prague by Herr Clemens Neumann. The author has observed, by very different and partly very singular methods, both the movement of particular points in the strings and also the movements of the entire strings.

The Temperature of Lakes.—At the same meeting of the Vienna Academy as that above mentioned, Professor F. Simony presented a comparative view of the temperature relations of the Hallstatter Lake, Gmunder Lake, and the two Langbath lakes, in which he had taken the temperatures at all depths at the same periods of the years 1868 and 1869, with the view to make out the extent of the influence of the different climatic characters of these two years on the temperature of lakes.

ZOOLOGY AND COMPARATIVE ANATOMY.

A New Stentor.—At the meeting of the Royal Microscopical Society (March 9), a paper was read from Mr. Barrett, M.R.C.S., in which a very curious new animalcule was described, which appeared to be like a rotifer which had found its way into a tube, but which the author believes to be a *Stentor*. We believe it is to be described in the April number of the *Monthly Microscopical Journal.*

The Spicules of Gorgonaceæ.—Mr. Kent, in a paper published in the *Monthly Microscopical Journal* (February), demonstrates that the microscope may be well employed in grouping this division of the *Actinozoa*. Mr. Kent's paper is accompanied by two handsome plates which fully bear out his views.

Have Animals Instinct or Reason?—Readers of Mr. Darwin's work who may admit instinct will still be led to give it such a philosophical explanation that it becomes but a form of reasoning under peculiar circumstances. To such we commend a very interesting paper on the "Intelligence of Animals," read before the Montreal Natural History Society by Professor Bell (January 31). The author spoke of the reasoning powers in many of the higher and larger animals as being too well established to require a plea, and devoted much of his paper to instances of what might be regarded as intelligence in such small creatures as insects. He adduced many arguments based on the organisation and development of these animals, and more especially on their habits, for regarding them as possessed of something more than real instinct. It was mentioned, amongst other proofs of the possession of a reasoning power, that insects, if baffled in one means of accomplishing their object, will generally try another; and that we find them as prompt and skilful in overcoming exceptional and artificial difficulties as in performing the ordinary duties of their lives. The habits of insects, like those of larger animals, appeared to be in a great measure the result of the accumulated experience of many generations. The term "instinct" has too general and vague a signification, and was often used as a convenient means of accounting for what we found it difficult to explain.

How to Preserve Larvæ.—Some practical but we must confess cruel directions are given in the last number of the *Entomological Magazine* by Mr. H. Pryer. Nevertheless, the method may be useful to many of our readers. It is as follows:—Having procured a larva, immerse it in a solution of alum or pyroligneous acid for a short time; then gently squeeze the inside out, upon or between pieces of blotting-paper, taking care not to tear or enlarge the anal orifice. After having extracted all the moisture and intestines, insert in the aperture a bent glass blow-pipe, having a bulb in the middle, and inflate the larva over a spirit-lamp, having a flat piece of zinc fixed over the flame. Holding the tube so that the larva does not touch the zinc plate, blow gently, until it is quite dry and hard. This requires great care, as it is liable to scorch; and, if the skin be only partially dry, it will, although apparently finished, become indented or depressed in those places where it is not thoroughly hardened. To ascertain if the operation be complete, cease blowing, and draw in the breath gently; when, if the larva is not dry, it will shrivel up, and must be inflated again immediately

if, on the other hand, it is dry, it will remain distended to its natural size and appearance. To prevent the head from being extended too much, dry all the body except the first and second segments, take the larva away from the spirit-lamp, and press the head into its natural position, afterwards drying it at some distance from the flame, without blowing through the tube. If the larva require colouring (as almost all green larvæ do), some very finely powdered dry colour (rather darker than the original colour of the larva) must be introduced into the skin after it is thoroughly dry. To get the colour evenly on the inside of the skin, it must be rolled about in the hand. In order to direct the heat on to any particular place, a small hole (about the size of the head of a pin) should be made in the centre of the piece of zinc. This would be found to be particularly useful, when finishing the head of the larva, as above described. The object of having the glass tube bent is that the head is then above the level of the flame; and the bulb is to prevent any moisture from the mouth running down the tube into the inside of the larva. I generally put a piece of blotting-paper in the mouthpiece of the tube.

A Crocodile in America.—*L'Institut* of March 23 announces the discovery in America of a new species of crocodile, differing from *C. acutus* and *C. rhombifer* found in Cuba. Professor Jeffries, who founds the new species on a skull given him by the person who killed the animal on the banks of the Miami, has given the measurements of the bones in detail.

Animals Eaten in Calabar.—At a recent meeting of the Zoological Society, some food curiosities were illustrated by Mr. Andrew Murray, who laid before the members specimens of edibles sold in the markets of Old Calabar. Amongst them was a fruit-eating bat (*Pteropus*) ready trussed for cooking, a crustacean rare in naturalists' eyes (*Callianassa Turnerana*), and the larva of a beetle found in the trunks of decayed palm-trees.

The Zoological Record and the Land Snails of New England.—At one of the last meetings of the Boston Society of Natural Science, Mr. Edward S. Morse called attention to a statement in the *Zoological Record* concerning a series of articles by himself on the Land Snails of New England, in which the reviewer remarked that he had returned to the system of Lamarck and Pfeiffer in the classification of these animals. The articles in question being published in a popular magazine were necessarily rendered as simple as possible, and divested of all technical details, and on this account only the earlier nomenclature was used. Mr. Morse disclaimed the intention of abandoning the position he had previously maintained, that these animals were divisible into natural groups by the structural peculiarities of the principal parts of the animal, such as the character of the lingual membrane, the form of the buccal plate, &c. While in general he had followed Albers in these subdivisions, he felt that he could not have adopted all of his genera without open violation to the natural characters of the animal, and though strongly tempted to follow the example of other systematists and include extra-limital species in the new genera proposed, had refrained from doing so, since he had had no opportunity at that time of going fully into the matter.

An Australian Fossil Crocodile.—Professor Owen has recently received, with remains of *Diprotodon*, etc., from the lacustrine deposits, Darling

Downs, Australia, portions of a crocodile, in similar fossil condition, which he determines to be identical with the species of crocodile still existing in the Queensland rivers.

The Thanophidia of India.—Dr. Fayer of Calcutta commenced in the *Indian Medical Gazette* of January 1, the first of a series of papers on these snakes. He illustrates his subject with numerous small diagrams indicating the specific peculiarities.

Hymenopterous Parasites.—Dr Hagen, an American naturalist, states that in a recent letter to him Professor Ratzeburg said he had carefully studied "Ichneumonosis," or the prevalence of hymenopterous parasitism, in the insects injurious to forest trees, and found that for many years it had carried off ten per cent. of the number of such insects. In 1867 and 1868, years in which the forests had suffered unusually from obnoxious insects, this ratio had been reduced to between one and two per cent., while at the same time "Mycetinosis," or the prevalence of fungoid parasitism, had increased to between forty and fifty per cent.; a balance of destructive power seemed to be always maintained between the two forms of parasitism. Mycetinosis had especially checked the ravages of the very destructive caterpillar of *Bombyx pini.*

Structure of the Pennatulidæ.—In connection with Mr. Kent's paper above referred to, we may mention that Professor Kölliker, who is still pursuing his researches on the histology of the Cœlenterata, has just issued a new part of his work on the subject. This is devoted especially to the *Pennatulidæ.*

A New Genus of Entozoa.—At the meeting of the Zoological Society on January 13, a communication was read from Dr. Cobbold containing the description of a new generic type of Entozoa, discovered in a specimen of the Aard-Wolf (*Proteles cristatus*), which had recently died in the menagerie. To this were added remarks on the affinities of this Entozoon, especially in reference to the question of parthenogenesis.

The New Zealand Mud-fish.—At the meeting of the Wellington Philosophical Society (New Zealand), on November 13, Dr. Hector called attention to two live specimens of the mud-fish from Hokitika, *Neochanna apoda* of Gunther. The specimens were swimming actively in clear water, and had perfect vision, although their eyes are small, so that the undeveloped state of the eye in the specimen previously received must have been exceptional. The Hon. Mr. Fox remarked that these mud-fish were not peculiar to Hokitika. Five years ago he remembered seeing a fish dug up from a gravelly clay ten feet below the surface at Rangitikei, and he believed that it was identical with the fish exhibited.

The Brighton Aquarium promises soon to be an accomplished fact. The site of the Aquarium, which will be upwards of 700 feet long and 100 feet wide, commences at the toll-house of the Chain Pier, and extends to within a few yards of that structure. On the south side there will be a sea-wall and an approach road, towards which the Corporation of Brighton have contributed 7,000*l.* The most complete arrangements will be made to bring under observation living sea and fresh-water animals and plants of various kinds, and to facilitate the study of their habits and structure.

A Shower of Shell-fish.—Our authority for the following account is a

recent number of the *American Naturalist.* Mr. John Ford exhibited to the Conchological Section, Academy of Natural Sciences, Philadelphia, specimens of *Gemma gemma*, remarkable as having fallen accompanied by rain, in a storm which occurred at Chester, Pennsylvania, on the afternoon of June 6, 1869. The specimens were perfect, but very minute, measuring one-eighth inch in length by three-sixteenths inch in breadth. Though most of the specimens which fell were broken, yet many perfect ones were collected in various places, sheltered from the heavy rain which followed their descent. A witness of the storm, Mr. Y. S. Walter, editor of the *Delaware County Republican*, assured Mr. F. that he noticed the singular character of the storm at its very commencement, and, to use his own words, "it seemed like a storm within a storm." A very fine rain fell rapidly, veiled by the shells, which fell slower and with a whirling motion. Judging from the remains of animal matter attached to some of the specimens, together with the fresh appearance of the epidermis, it is highly probable that many of them were living at the moment of transition. This minute species resembles a quahaug shell, and is common on the seashore between tide marks.

POPULAR SCIENCE REVIEW.

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J. ALDERMAN,

16 SOHO SQUARE, LONDON.

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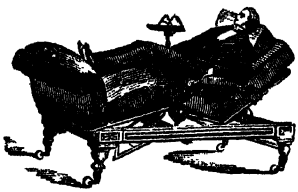
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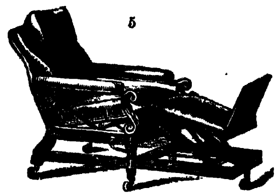
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The following gratifying Letter has been received by MR. ALDERMAN *from* GENERAL GARIBALDI.

'Caprera, August 20th, 1863.

'MR. ALDERMAN,—Some time ago I addressed to you a few words of thanks for your magnificent bed, which has been so great a comfort to me dur ng my illness. Now I hear with regret that my letter has miscarried, and I willingly come forward again to express my gratitude to you. You have alleviated half my sufferings. My affectionate thankfulness will accompany you eternally. Accept a grasp of the hand from yours ever,

(Copy) 'G. GARIBALDI.'

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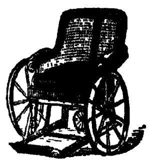
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ON THE SPHÆROSIRA VOLVOX OF EHRENBERG.

By Professor WILLIAMSON, F.R.S.

[PLATE LXI.]

WHEN the well-known aphorism of *omne vivum ex ovo* was displaced by the more recent one of *omne vivum ex cellulo*, it was thought, by many advocates of what has long been called the cell theory, that physiologists had reached the most elementary form of organisation. But how far this is from being true is shown by the vast advance in knowledge of the subject which has taken place during the last twenty years. We have learned that what was thought to be the all-important cell is now but a comparatively insignificant product of the true generating organism; an incidental portion which may or may not be present, but the presence or absence of which has little, if any, influence upon the active protoplasmic element in which resides the true life, and by which is fulfilled the important functions of growth and reproduction. The outer cell membrane resolves itself mainly into a mere framework, secreted by the active protoplasm, and designed to provide a suitable protective chamber in which the chemical and physiological operations of the protoplasm can be conducted without disturbance from external forces. Beyond this protective function, it is doubtful if the cell-membrane fulfils any purpose save that of the membranous analyser regulating some of the osmotic phenomena of which all cells are the seat. The true phenomena of cell-life reside in the protoplasm, and if there is any truth in the doctrine of pangenesis, we may yet have to fall back upon the minute granules which abound in protoplasm as the true reproductive germs, endowed with a self-sustained vitality, and capable, under certain circumstances, of an independent life and action.

Since physiologists arrived at these conclusions many of the difficulties attending the study of the microscopic algæ, once so formidable, have disappeared like the dew before the

morning sun. Facts which appeared to be anomalous are now arranged in their proper places. The action of the protoplasm, independent of an investing cell-wall, can be studied in a thousand forms, and every such study is replete with interest. But in its progress this enquiry has developed a cognate one, and demonstrated that in many of the varied forms assumed by objects once thought to be distinct from each other, we have but so many manifestations of vital phenomena in individual species. Hence, rapid as has been the increase in the number of recorded specific forms of plants and animals since the publication of Ehrenberg's great work, it is questionable whether that increase has not been more than neutralised by the synthetic process by which it has been counterbalanced.

This synthetic aggregation of forms has been especially remarkable in the case of the *Volvocinæ*. That *Volvox globator*, *aureus* and *stellatus*, are but modified forms of the first-named species, is now an almost universally recognised truth. More difference of opinion, however, exists in reference to the curious little organism named by Ehrenberg *Sphærosira Volvox*. Towards the middle of April in the present year the *Volvox globator* appeared in marvellous abundance in a pond near my present abode, and amongst the common forms were myriads of *Volvox aureus*, and a considerable number of the *Sphærosira*. About the 10th of May some cold, wet weather arrived, and the whole stock disappeared from the pond; whilst of those which I was preserving at home no trace now (May 7th) exists, save vast numbers of the spores of *Volvox aureus*, which remain quiescent at the bottom of the glass jar in which they were contained. I availed myself of the opportunity thus afforded me of subjecting the *Sphærosira* to a rigid examination. The specimens were about the same size as the ordinary *Volvox*, but were more egg-shaped and rather longer in one direction than in the opposite one. This peculiarity, however, I found to be shared by many of the specimens of the common type of *Volvox* found in the same pool, though in a less marked degree. As is characteristic of the form under consideration, the separate masses of protoplasm scattered over the sphere varied in size; most of those at one end of the organism (fig. 1, *a*) being small, and the greater number of them uniform in size, being altogether undistinguishable from those of the associated *Volvox*. It has been affirmed, by Ehrenberg, that a distinction exists between the *Sphærosira* and the *Volvox globator*. He says that in the former each protoplasm is supplied with one cilium, whilst in the latter there are two. But I am convinced that this is not the case. True, I only found one cilium attached to each gemmule of *Sphærosira*, but I also found this

to be the case with nearly the whole of the individuals of *Volvox globator* in the same pond, demonstrating that, even in the latter, the number of the cilia is not a constant element, and that, consequently, it cannot be relied upon even as a specific, much less as a genuine, ground of distinction. This fact also explains the discrepancies hitherto existing in the observations of Ehrenberg, Dugardin, and Busk. Ehrenberg, as we have seen, separated *Sphærosira* from *Volvox*, on the ground that the former had one cilium to each gemmule, or gonidium, whilst the latter had two; but the other two observers just named deny the correctness of the statement on the ground that two cilia exist equally in both the forms. It is now evident that the number of these appendages varies both in *Volvox* and in *Sphærosira*. As we approach the opposite end of the sphere (fig. 1, *b*), we find that some of the gemmules begin to increase in size, the enlarged ones being apparently distributed without any special order or arrangement amongst those which remain comparatively unaltered. But as we advance, we find that nearly all the gemmules are either changed or changing into large compound masses—these enlarged clusters of protoplasms occupying something like two-thirds of the entire sphere. On watching their development, we discover that they grow in precisely the same way as I, some years ago, demonstrated to be the case in *Volvox globator*. At first, one of the ordinary protoplasms—which, by the way, have been variously designated by the terms gemmules, gonidia, and ciliated zoospores—becomes slightly enlarged (fig. 2). It then appears as a green mass containing a few large and distinct granules in the midst of numerous very minute ones, diffused through the protoplasmic base. This mass soon divides by fission into two (fig. 3), and this again successively into four (fig. 4), eight, sixteen (fig. 5), and thirty-two (fig. 6), beyond which the fission was not carried in any of the examples which I examined. It thus appears that five successive acts of fissiparous segmentation took place, every one of the enlarged protoplasms undergoing division at each repetition of the process. So far, all this corresponds with the ordinary way in which the gonidia develop into the young spheres that give such beauty to the *Volvox globator*; yet a marked difference is seen in the results. In the *Volvox*, these successive divisions end in the formation of a small *sphere*—a miniature representative of the parent globe—but in *Sphærosira* the product is a *flat disk*, in which the aggregated and elongated protoplasms are arranged vertically to its surface (fig. 7), constituting an organism which, in a free state, closely resembles those types which Ehrenberg has elevated into genera under the names of *Euroglena*, *Syncrypta*, *Uvella*, and

Synura. The differences in the results just referred to arise mainly from corresponding differences in the direction of the cleavage in the successive fissions. In *Volvox*, we have the first cleavage into two protoplasms, taking place *vertically* to the surface of the parent sphere. The second also takes place vertically, but in a direction at right angles to the first. The next division appears to take place along a plane *parallel to* the surface of the parent sphere. Hence, as soon as we obtain eight distinct protoplasms, we find them arranged in two parallel layers, the thin space between the two representing what is ultimately destined to become the hollow interior of the young sphere. In *Sphærosira*, on the other hand, though the fissions change their direction, they result in the formation of a disk, and not in a double series of protoplasms, because they are all made vertically to the surface; the horizontal ones of the ordinary *Volvox* not taking place. When Mr. Carter pursued his observations on the subject, he thought he had found examples in which each disk consisted of 128 ciliated segments. If this is true, and I see no reason for doubting it, segmentation in his case had been repeated seven times, whilst in mine they were limited to five. In both instances the process was completed, since, in both, the separate protoplasms became provided with cilia—a stage of growth which, so far as my experience enables me to judge, always indicates the completion of the segmentative process, and the final determination of the number of gonidia into which the mass is destined to be divided. The young organism now assumes a new character. Hitherto it has been passing through a stage of still life; it is now furnished with locomotive organs. If we examine carefully one of the disks in this completed stage, we shall find that it exhibits appearances represented in figures 6 and 7. In the former, we have a flat disk consisting of thirty-two green protoplasms, looking somewhat like some forms of the genus *Pediastrum.* Fringing this disk, we can trace a number of freely moving cilia (6, *e*). Each protoplasm, in this respect, resembles the parent gonidium (fig. 2) from which it originated; but as we watch, we observe that the disk rotates slowly; in one instance I was able distinctly to trace the walls of the mother-cell (7, *d*) within which it was imprisoned. In this movement of the disk within the interior of the mother-cell we have another feature of affinity with *Volvox*—in which latter, as I long ago pointed out, a similar movement of the young imprisoned sphere may occasionally be observed. In the *Sphærosira* this revolving motion affords us facilities for observing the lateral aspect of the compound disk, and of the individual protoplasms composing it. We now see that each of the latter is elongated, and slightly curved. The centre of

the upper surface of the compound disk (fig. 7, *a*) appears to be a little depressed. The lateral concavity of the curved protoplasms (7, *b*) is in all cases directed inwards. Throughout the greater part of its length each protoplasm exhibits the same green colour and numerous bright granules that appear in its upper surface; but its lower extremity (fig. 7, *c*), which before movements began was directed towards the centre of the parent sphere, is colourless; and from it is projected apparently a single cilium (7, *e*). From their rapid motion I found it impossible to count them; and even when aided by tincture of iodine and other reagents the result was equally a failure; but my impression was, and is, that each protoplasm bore a single cilium, not two. In this stage of its development, each protoplasm bears the closest possible resemblance to an *Euglena*—the motile condition of *Protococcus pluvialis*. I am convinced, that if one of the gemmæ of *Sphærosira* and an individual *Euglena viridis* were placed side by side, they would be wholly undistinguishable. By what means the several protoplasms were bound together I was unable to ascertain. Neither did I succeed in tracing their ultimate condition after the *Volvocinæ* as a whole disappeared from the jar. As already observed, when they began to do so, they vanished rapidly, and nothing was left in the water indicative of the myriads which it contained a few days before but numerous spores, if such they are, of *Volvox aureus*. When I found that this was the case, I visited the pond whence the specimens were obtained on May 26. Not one solitary example could be found either of *Volvox* or of *Sphærosira*; neither could I discover a trace of *Uvella*, or of any of its compound allies which could be identified as having been developed out of a *Sphærosira*. A few examples of Euglena longicauda were the only moving forms of protoplasm that presented themselves. I then turned my attention to the mud at the bottom of the pond, collecting its surface-layer by means of a glass tube. Spores of *Volvox aureus* in various conditions were not unfrequent, but nothing like the young compound gemmæ of *Sphærosira* were to be seen. There was every reason to conclude that the latter were as temporary and fugitive in their nature as the young spherical gemmæ of the common *Volvox*, and, like them, had wholly passed away. What then are the compound gemmæ of *Sphærosira*?

Mr. Carter believes them to be a spermatic form of *Volvox*, each separate Euglenæform organism being a true spermatozoid. I confess I see no reason for accepting this determination. I have been unable to detect in these objects any evidence of sexuality, and am more disposed to conclude, with Professor Busk, that *Sphærosira* is merely a *Volvox* in which

the development takes place in a peculiar way. The peculiarity is twofold. First, instead of some half-dozen of the gonidia being selected for fission and development into spheres, as in *Volvox*, in *Sphærosira* fully one-half of the entire number contained in the organism are developed. Then there is, secondly, a difference in the direction of fission. In *Volvox*, as already seen, this fission not only takes place along various planes *vertical* to the surface, but also in others that are horizontal to and parallel with it. In the *Sphærosira* we have the former system of cleavages, but not the latter. Hence the separate protoplasms are grouped in a single plane with their long axes vertical to the surface of the tabular disk. This limitation of the direction of cleavage thus reduces itself to a comparatively small matter, and appears to me not only to indicate no generic distinction, but not even a specific one. It merely exhibits one of the incidental variations in growth and development so common amongst these lower algæ, and, regarded physiologically, has no special value; least of all does it appear to bear any relation to the sexual reproductive processes so common amongst the higher cryptogamia.

THE USEFULNESS OF THE FIFTH IN MUSIC.

By the Rev. C. HOPE ROBERTSON, M.A.

I.

IN amateur musical work, either for home use or for training a choir, difficulties are often found about the different keys in music, their signatures, and rules for transposing pieces up or down, as may be needed for suiting various voices.

Now some most useful helps may be got on these points by an examination of the musical interval of the fifth, from which by a little cross-questioning we may draw some rules for popular use which are not generally found in musical treatises. These, in general, give directions without explanations of their *modus operandi.*

We therefore propose to make the *interval of the fifth* in music perform a little "musical magic," and tell us these points:—

1. In what order different keys follow each other.
2. What notes are to be made sharp or flat in the signatures, and in what order they follow each other.
3. How to know by inspection what keys pieces are written in, and into what they may be transposed.
4. What changes have to be made in the signatures of keys to transpose pieces up or down, any number of notes.

II. We must first, for the sake of clearness, recall to mind a few definitions of musical terms necessary for our purpose.

The ear teaches us that a certain sequence of notes rising up from the starting note or key note, such as C, must follow each other in a certain regular course, if a pleasant effect is to be made, and this succession of notes is called the major scale of that note. It comprises eight notes, the last being the octave above the key note. It is found on examination that the interval between these notes are as follows:—First, 2 whole tones, then 1 semitone; next, 3 more whole tones and 1 more semitone: or to put it in a series of letters, marking the semi-

tones with a circumflex, we get this order for a major scale of C—

C, D, E, F, G, A, B, C,
1, 2, 3, 4, 5, 6, 7, 8;

where the semitones must occur between the 3rd and 4th, and between the 7th and 8th, notes of the scale. This is the case for any of the other notes, if made the starting or key note of a piece of music. But in the case of any other key note except C the semitones will therefore need rearranging, which is done by adding sharps or flats to the notes of the scale until the right succession of tones and semitones is got. If we take G for instance as the key note, and rise to an octave above it, the first semitone between its 3rd and 4th note happens to coincide with one already there in the scale of C, but the 2nd semitone, to be between the 7th and 8th notes, requires to be made by sharpening the 7th note, F. We thus have the right succession of tones and semitones for the scale of G by making one note sharp. This mark of F sharp is put once for all at the beginning of any piece of music written in the scale of G, and such mark is called the signature of the key.

In a similar way we must alter by sharps or flats the notes of the scale, for any key note we need to write music in, until we have got the right arrangement for the major scale of that particular note. At present minor scales, which have a different arrangement of the tones and semitones, are not considered.

III. The musical interval called a fifth consists of three whole tones and one semitone. For instance, between C and G there is a fifth, G being the 5th note above C, counting each of them:—

C, D, E, F, G.
1, 2, 3, 4, 5.

In this case the natural arrangement of tones and semitones is right. But, if needed, the notes would have to be altered by sharps or flats to make the interval perfect.

Now we may go to work with this interval, and see what we can get out of it in various ways.

IV. If we write the natural scale of C several times over in succession, and rise from the starting note by jumps of a fifth each, we get a series such as the following:—

CDEFGABCDEFGABCDEFGABCDEFGABC
1 2 3 4 5 6 7

Here G is the first fifth above C; D the next above G; and so on, till we come to another C, at the 7th fifth above the C we started from.

Now, putting these notes for the fifths in regular order by themselves, we get this series :—

Series I.

C, G, D, A, E, B, F, C,

which succession of notes is to do wonders for us. It is a series which it is well to learn by heart, to repeat when needed, either forwards or backwards.

But before using it, it needs a little adjustment; for, like many other things, it is not perfect at first. On inspecting the position of the semitones in the long succession of notes we took it from, we see that the first 5 fifths are right in their tones and semitones; but the sixth would contain 2 semitones, those between B C, and E F; therefore we must make F sharp to perfect this fifth. But that will derange also the next fifth, which will require its last note, C, to be similarly sharpened, that the interval may be perfect between F sharp and C sharp.

Series I., when thus corrected in these points, becomes

Series II.

C,	G,	D,	A,	E,	B,	♯F,	♯C,
0,	1,	2,	3,	4,	5,	6,	7.

V. Now, if we continued this series of fifths still further above this C sharp, we should have 5 more fifths, coming at last to one on B sharp. But B sharp is the same as C natural. So that at the 12th fifth we return into the same key note we started from, and the fifths would only repeat themselves again if we went further. The complete series of fifths is thus made :—

Series III.

C,	G,	D,	A,	E,	B,	♯F,	♯C,	♯G,	♯D,	♯A,	♯E,	♯B (or C).
0,	1,	2,	3,	4,	5,	6,	7,	8,	9,	10,	11,	12.

VI. We may notice in passing, that in the tuning of a pianoforte this high C, got by rising up 12 fifths, does not exactly coincide in sound with the C got by rising towards the same pitch by octaves from C. The high C got by fifths is a little higher than the C got by octaves; and hence the necessity, in the actual tuning of the piano, of flattening all the fifths *a very little*, in order that the two Cs may coincide.

VII. Leaving now for a little this *Series III.* of fifths ascending from C, let us examine what would be the order of a *falling series* of fifths descending from C. Taking a similar long succession of notes, but reading them backwards from the top C, we should get our fifths as before.

CDEFGABCDEFGABCDEFGABCDEFGABC

7 6 5 4 3 2 1

We get in the order marked by figures,

SERIES IV.

C, F, B, E, A, D, G, C.

Now, on inspecting this series, it is seen that they are the *same notes, reversed in order*, which were got in Series I.

This (Series IV.) is, however, like Series I., not quite complete at first. Its 6 last fifths need alteration. For in its 2nd fifth 2 semitones occur, and we must therefore get another semitone to perfect that fifth. This is to be done by *flattening* its last note, B, one semitone, which cuts into the succeeding fifths and requires their last notes to be flattened also. The Series then becomes

SERIES V.

C, F, ♭B, ♭E, ♭A, ♭D, ♭G, ♭C, (or B).

Here C flat being the same as B natural, we may take B instead of C flat; and if we continue the fifths below that, we get the following complete series, ending at last on another C natural; after which the series of fifths would only repeat themselves.

SERIES VI.

C, F, ♭B, ♭E, ♭A, ♭D, ♭G, B, E, A, D, G, C.
0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12.

Thus, in either rising (Series III.) or falling (Series VI.) by fifths from C natural we come in each case again to a C natural, *after* 12 *fifths*.

VIII. We are now in a position to apply this to practical results. Taking Series II., if I wish to know what keys require 1, 2, or more sharps to indicate them in their signatures, this Series II. gives what we want.

For it is found on trial that

The key of C has 0 sharps.
" " G has 1 sharp.
" " D has 2 sharps.
" " A has 3 sharps.
" " E has 4 sharps.
" " B has 5 sharps.
" " ♯F has 6 sharps.
" " ♯C has 7 sharps.

Each note therefore in Series III. *needs* 1 *sharp more than* the note preceding it.

Now we may write the notes with the number of sharps belonging to them underneath, and get this order :—

(Notes)	C,	G,	D,	A,	E,	B,	♯F,	♯G.
(Sharps)	0,	1,	2,	3,	4,	5,	6,	7.

It is evident that here we at once can see how many sharps any of these notes requires; and if we see a piece of music with several sharps or flats at the beginning we can tell easily, from this set of notes, which key it is written in. As, for example, a piece with 4 sharps as its signature is written in the key of E, the 4th sharp above C. More than 7 sharps are not needed generally, as they would then be more easily expressed otherwise in a way we need not now go into.

Or the piece could be transposed into a key having a smaller number of sharps.

IX. But, further, we might want to know *which notes* in the signature of any key having sharps *are to be so marked*, and *in what order* they come after each other. Series II. gives a rule for this also.

For it is found on trial that the notes requiring to be sharpened are as follows:

The key of	C has 0	sharp.			
„ „	G has 1	sharp, which is F.			
„ „	D has 2	sharps, which are	F, C.		
„ „	A has 3	„	„	F, C, G.	
„ „	E has 4	„	„	F, C, G, D.	
„ „	B has 5	„	„	F, C, G, D, A.	
„ „	♯F has 6	„	„	F, C, G, D, A, E.	
„ „	♯C has 7	„	„	F, C, G, D, A, B, E.	

(SERIES VII.)

Now here we see that the notes to be marked sharp follow each other in a Series (VII.) which is just similar to Series II., with the addition of one F at the beginning; being the first fifth below C.

The *notes to be marked sharp* are thus themselves *a series of fifths*, but beginning 2 *fifths* below the key note they signify.

Series II. therefore gives both the succession of sharp keys, and also of the notes to be marked sharp in their signatures.

X. Let us take now some examples of these uses of Series II. and VII. If I see a piece of music marked with 5 sharps, and want to know *what key it is in*, I at once run over in my mind these notes of Series II.,

C,	G,	D,	A,	E,	B,	♯F,	♯C,
0,	1,	2,	3,	4,	5,	6,	7,

and see it is B that has that number of sharps—standing, as it does, 5 fifths above C.

Or if I was told to write a piece of music *in the key of B*, my series would tell me it needed 5 sharps as signature; the notes these 5 sharps *are to be placed on* are got by taking the first 5 notes in the Series VII., and so on in the case of any number of sharps.

Another useful rule may be derived from inspecting the order in *Series VII.*

It may be seen that, if a certain number of sharps is marked as the signature of the piece of music, the key note is the *next note* of the *natural scale above the last sharp* marked as its signature. Thus, music written in 1 sharp, which is F, is in the key of G; if in 2 sharps, they are F and C, and the key is D, &c.

XI. With regard to the *mode of writing the signatures of keys*, a useful rule is got by dividing the series of fifths (II.) into two columns alternately, thus:—

0	C	1	G
2	D	3	A
4	E	5	B
6	♯F	7	♯C

Placing the number of sharps needed by these key notes beside them, we have got the odd and even number of sharps in separate columns. The notes in these columns are now seen to rise above each other in 2 series of natural scale intervals, beginning from C and G respectively. Now, in each of these columns the notes require *each* 2 *sharps more* than the one before.

From hence arises this plain rule, if *I wish to transpose a piece one note higher, I must add* 2 *sharps* to the signature, in order to put it into the right key for its new pitch. And so on for each other note it is raised.

XII. Again, separating Series VII. into two similar columns, we get:—

F	C
G	D
A	E
B	F

Here we see the *sharps in signatures* may be written in two columns of notes in natural succession, starting from F and C as the first two sharps, and putting each fresh sharp alternately above these two notes.

The inspection of any signature of music written in several sharps will show that this is done. Our examination of Series VII. shows *why* it is done.

XIII. The examination of the *flat keys* is to be conducted in a similar manner as the sharp keys.

Taking Series V., in which we got the order of fifths falling

below C, we can gather from it *what keys* require 1 *or more flats* to mark them at the beginning of a piece of music.

For it is found on trial that

The key of		C	has	no flats.
„	„	F	„	1 flat.
„	„	♭B	„	2 flats.
„	„	♭E	„	3 flats.
„	„	♭A	„	4 flats.
„	„	♭D	„	5 flats.
„	„	♭G	„	6 flats.
„	„	♭C	„	7 flats.

Each note in the Series V. has therefore 1 *flat more* than the note before it.

Writing the notes and their flats under them—thus,

C, F, ♭B, ♭E, ♭A, ♭D, ♭G, ♭C,
0, 1, 2, 3, 4, 5, 6, 7,

we see at a glance what number of flats is needed for each note.

More than 7 flats need not practically be taken.

XIV. Further, if we want to know *which are the notes* that *in the signature* of these flat keys are to be marked flat, and in what order they come, we take the descending series of fifths, and find by trial that

The key of		C	needs	0 flat.			
„	„	F	„	1 flat, which is B.			
„	„	♭B	„	2 flats, which are B, E.			
„	„	♭E	„	3	„	„	B, E, A.
„	„	♭A	„	4	„	„	B, E, A, D.
„	„	♭D	„	5	„	„	B, E, A, D, G.
„	„	♭G	„	6	„	„	B, E, A, D, G, C.
„	„	♭C	„	7	„	„	B, E, A, D, G, C, F.

(Series VIII.)

Hence we see that the notes themselves to be marked flat in signatures are also a *series of Fifths*, and they are the same *notes read backwards* as the notes of Series VII. for sharps.

From this succession we can at once allot the right number of flats, and place them on their proper places.

If, for example, a piece of music is written in 5 flats, and I want to know what key it is in, I run over the series of fifths descending from C, and take the 5th one, ♭D; or, if I was told to write a piece in the key of ♭D, I see at once from the series that 5 flats must be marked in the signature, and that these notes are the first 5 of Series VIII. So in like manner for any other number of flats.

XV. A useful rule for knowing at sight, by inspection of the flats, what key a piece is written in, is also got from Series VIII. There it can be seen that the key note is *a fourth below the*

last flat. This (if there are more than one flat) will coincide with the second last flat.

XVI. Writing the series of flat fifths in two alternate columns, as we did the sharp ones, they will form two sets of notes falling in natural succession from C and F alternately—thus:—

0	C	1	F
2	♭B	3	♭E
4	♭A	5	♭D
6	♭G	7	♭C

The number of flats required by these key notes being placed beside them, it is evident that if a piece is *to be transposed lower* in pitch, we must *add 2 flats in the signature* for each note it is lowered, in order to get the right signature for the key of the new pitch it is to be put into.

This, in the case of a piece already written in sharps, would be the same thing as *taking off* two of the sharps.

Similarly in the case of raising a piece a note, which we spoke of in considering the sharps—instead of adding two sharps—if we had the piece originally written in a flat key, the same result would be obtained by *taking off* two flats.

XVII. Another useful rule may be got by comparing the two successions of sharp and flat fifths.

SERIES IX.

(Sharp Keys) . . .	C,	G,	D,	A,	E,	B,	♯F,	♯C.
(Flat Keys) . . .	C,	F,	♭B,	♭E,	♭A,	♭D,	♭G,	♭C.
(Number in *signature*)	0,	1,	2,	3,	4,	5,	6,	7.

Examining this arrangement, it is seen that if any key-note has a certain number of sharps, the *same note flattened* has a number which is the *difference* between that number and 7, as its allowance of flats; or, the signatures in sharp keys are the complements to 7 of the signatures in flat keys:—

Thus { G has 1 sharp.
  ♭G has 6 flats.

" { D has 2 sharps.
  ♭D has 4 flats.

" { A has 3 sharps.
  ♭A has 4 flats.

&c. &c.

Now from this it results that, without *altering the notes* in which a piece is written, we may at once transpose it a semitone lower or higher by playing it in the complementary number of flats or sharps. Thus, a piece written in 3 sharps may be played as if in 4 flats from the same written score (except any accidental sharp or flat it may contain), and this will lower its whole pitch one semitone.

Or, if written in 4 flats, it might be raised a semitone by being played as if written in 3 sharps, without needing to alter the score (except in any accidental sharps or flats it contains).

Often, in instrumental playing, sharp keys may be easier than flat ones, or *vice versâ*, and an immediate change can thus be effected; or in singing, the voice may want a little easing in pitch, which is thus easily given.

These results we have arrived at from the useful succession of fifths will, it is hoped, clear the way in sundry difficulties of home music, adding to its easy understanding and so to its pleasure.

THE ANATOMY OF THE RIVER-MUSSEL.

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[PLATE LXII.]

"Molluscorum scilicet testas inhabitantium anatomiam instituere, eorumque internam structuram, partium formam et compagem, situs, nexus, atque usus declarare; sive, ut verbo complectar, eorum zoologiam ac physiologiam simul persequi, opus certe pro rei dignitate eximium atque præstantissimum, prout est ceterorum omnium intricatissimum atque difficillimum."—POLI.

IN the POPULAR SCIENCE REVIEW for April 1869, the leading characters of the great sub-kingdom Mollusca were ably sketched by Mr. St. George Mivart, F.R.S., and illustrated by the anatomy of the cuttle-fish. In the present article it is proposed to give an account, as concise and accurate as possible, of the structure and zoological affinities of one of the classes of the above sub-kingdom, namely, the *Lamellibranchiata* (leaf-gilled molluscs), as the type of which the common river-mussel * (*Anodonta cygnea*) has been chosen.

The Lamellibranchs are the only representatives of one of the two divisions into which, according to Professor Huxley (*Hunterian Lectures*, 1868), the Mollusca may be conveniently divided, namely, the *Anodontophora*, or those which are devoid of any "odontophore," or tooth-bearing tongue; the *Odontophora* comprising the rest of the Mollusca, such as the snails and slugs which breathe by means of gills or of a pulmonary sac, the cuttle-fishes, and the oceanic "sea-butterflies," certain of which, as the *Clio Borealis*, form the principal part of the food of the baleen-whales.

* The various species of river-mussel, of which *Anodonta cygnea*, *Unio tumidus*, *U. pictorum*, and *U. margaritifer* are British representatives, have been classed together in a family termed, by poetical license, "Naiades"—river-nymphs. If a river-mussel shared our perplexity, it would doubtless exclaim, in the language of Keats:—

"Why it is thus, one knows in heaven above:
But, a poor Naiad, I guess not."

On the principle of "Ab uno disce omnes," the reader, it is hoped, will, after witnessing the dissection of a river-mussel, which I propose to perform in his presence, be in a position to appreciate the likenesses and unlikenesses which certain other members of the Lamellibranchiata have to one another, and to the representative which has been chosen to bear the investigation of the scalpel.

The Shell.—It will first be noted that the animal is the tenant of a shell—and not the shell the occupant of the animal, as in the cuttle-fish—consisting of two irregularly oval folding-doors or "valves," of like size and shape, connected together along one margin by a sort of hinge. It is, in fact, what conchologists term a "bivalve."

It will naturally be asked which is the right and which the left valve of the shell, and which the anterior and which the posterior aspect of a given valve.

On each valve may be seen a slight prominence—the "umbo"*—nearly surrounded by numerous concentric lines, and lying close to the edge which is hinged on to the neighbouring valve, and much nearer to one than to the other of its extremities.

If, then, the mussel be held with the hinge-margin uppermost, and the umbo away from the observer, that end will be anterior which is farthest from him, and that valve, the right valve, which is on his right hand, and *vice versâ*; the *dorsal* edge of either valve being that by which it is joined to its fellow, while the opposite unattached edge is the *ventral* margin.

It may be not amiss to state here that the bivalve shell of the Brachiopoda,† e.g. *Terebratula* and *Lingula*, does not answer to the lamellibranchiate shell, for its valves are dorsal and ventral instead of being lateral.

The shell is strictly a structure secreted by the "mantle" (M M′ fig. 1), that fleshy robe which invests the animal; being, so to speak, a hoarded, but not useless, lumber of cast-off clothing. Destitute of blood-vessels, it has no inherent power of growth or repair, but is dependent upon the mantle for any addition to its substance.

The secretion of the true shell, with its covering of epidermis, is carried on by that thickest portion of the mantle which constitutes its margin or "collar" (see lower part of left lobe M′, fig. 1), while by the thinner part (to which the letter M′ points) is formed the "nacre" or pearly lining. This

* Lat. *umbo* = the boss of a shield.

† Often called "lamp-shells," from their resemblance to an ancient lamp.

may be well seen by comparing the mode of repair of injuries at the margin with that of those nearer the umbo of the shell.

If the pearly lining be treated with a dilute acid, the carbonate of lime, of which it is partly composed, will be dissolved out, and a structureless organic membrane will be left behind. The late Sir David Brewster was the first to make known that the rainbow-hues of this "nacre" were due to the presence of grooved parallel lines on its surface, "produced by cropping out of laminæ of shell situated more or less obliquely to the plane of the surface." He, however, supposed that there was an outcrop of alternate layers of membrane and calcareous matter; but it has been since found out that there is but a single membrane disposed in folds lying more or less obliquely to the surface, and between which the lime-salts are deposited. This can be well shown by decalcifying the "nacre" of the shell of the "ormer" (*Haliotis*)—so common an article of food in the Guernsey markets—when the iridiscence will still persist; but, if the folds of membrane be smoothened out by needles, it will disappear.

The peculiar lustre of "nacre" is a good example of that interference of rays of light reflected from different surfaces, which is known by the name of "diffraction."

The pearls which are, or at all events, were procurable in the mountain-streams of Britain, from a kind of fresh-water mussel, *Unio margaritifer*,* as well as from the better-known pearl-oyster of the Indian Seas, consist of layers of nacre formed round "foreign bodies," which either through accident or design have been introduced into the shell; for the mussel and oyster, like the Sybarite fretting at the crumpled rose-leaf, abide not the presence of even a grain of sand.

Of this irritability of the mussel the Chinese have long taken advantage, by introducing, either through the outer shell, or between the nacreous lining and the mantle, various objects which they wish to become pearl-washed. In the Museum of the Royal College of Surgeons may be seen two valves of a *Unio*, brought from the neighbourhood of Ningpo, into which twenty-three plates of a white metal, each stamped with a figure of Buddha, have been introduced, and have been since coated with a layer of nacre.

The pearls differ in quality as they are developed in the outer or inner layers of the mantle; being, in the former case, very irregular, sometimes large, and adherent to the nacreous lining of the shell, while in the latter instance they are small,

* "In Britanniâ parvas atque decolores nasci certum est, quoniam divus Julius thoracem quem Veneri Genetrici in templo ejus dicavit, ex Britannicis margaritis factum voluerit intelligi."—Pliny, *Nat. Hist.* lib. ix.

regular in shape, and are imbedded in the soft parts of the animal.*

The French naturalist, Lacaze-Duthiers, having found in the ovary of a *Mactra* (one of the Lamellibranchs) whose renal organ contained concretions of uric acid, an ovum which included between its shell and yolk a veritable calculus, suggests that pearl-producing molluscs are in a condition like unto that of a gouty man; seeing that, as in the latter case, concretions of uric acid or its salts are apt to be formed in the bladder or around the joints, so, in the former instance, the excess of material over that which would normally suffice for the wants of the shell comes to be precipitated in the form of a pearl. "En un mot, le mollusque produisant des perles, n'est-il pas un être atteint d'une diathèse calculeuse?"

The true basis of the shell, a very small portion of which can be seen running round outside the edge of the nacreous layer, and which has a somewhat roughened appearance when viewed through a hand-lens, consists of a layer—the "prismatic"—made up of carbonate of lime deposited in elongated cellular cavities disposed vertically to the plane of the shell, and which are the result of a successive *piling up of perforated* laminæ, the polygonal holes in which correspond, or "coincide," as Euclid says, secreted periodically by the then thickened margin of the mantle.

A structureless epidermis—the "periostracum"†—consisting of organic matter only, covers the shell-tissue and protects it from the solvent action of the carbonic acid with which fresh waters are more or less charged. It is connected with the edge of the mantle, being secreted, like the prismatic layer, by it, and is, in certain Lamellibranchs possessing siphons, *e.g.* the "gaper" (*Mya*), continued over these organs. In *Anodonta* it is ornamented with concentric ridges corresponding to the lines of growth; but in another kind of fresh-water mussel—the *Dreissena*—strictly a native of Russia, but now naturalised, after emigration on foreign timber, in some of our rivers and canals, these lines radiate from the umbo to the circumference of either valve.

Professor Rolleston, from observing that the shells of *Anodonta* from the rivers around Oxford are thinner than those from mountain streams, though the latter waters are poorer in salts of lime than the former, infers that the amount of inor-

* In the educational series of Invertebrata contained in the Museum of the Royal College of Surgeons, there is a preparation (No. 60) of a *Unio* which has a well-shaped pearl, of about the size of a pea, imbedded in the right side of the foot.

† From περί = around, and ὄστρακον = a shell.

ganic matter which these organisms take up and deposit in their tissues, is "by no means dependent upon the amount present in the medium in which they live, but upon the selective working of their tissues."*

The mechanism of the hinge is very interesting. It consists of—1. The "ligament," a thickened modification of the periostracum, which is attached on either side to a ridge on the dorsal margin of the shell-valve, just posterior to the umbo, and is, consequently, being elastic, put on the stretch by the closing of the valves by the adductor muscle. 2. The "cartilage," spring, or internal ligament (*c*, fig. 1), is lodged on either side in a kind of furrow, bounded by two ridges, the outer of which gives attachment to the ligament. The inner ridge is indicated in fig. 1 by the letter *h*. This spring is composed principally of elastic fibres, having a direction perpendicular to the plane of the surface of attachment, and which are, consequently, compressed on the closing of the shell-valve.

Here we have a good instance of the economy of physiological labour; for while the opening of the shell, a condition favourable for the respiration and nutrition of the contained animal, is brought about by the purely physical means of an elastic tissue in a state, so to speak, of equilibrium, the closing of the valves, which is but an occasional act (rendered necessary by the presence of danger, &c.), is effected by muscular action, which involves the outlay of a certain amount of nerve force.

If this state of things were reversed, the animal would become speedily "tired," and the "waste of tissue" would be so much increased as to become quite prodigal.

Those acquainted with human physiology will, doubtless, call to mind the antagonism to the external intercostal muscles of the stretched rib-cartilages, and the contained lung tissue, in the mechanism of respiration.

Muscular System.—This is comparatively simple; comprising only the muscles of the shell and of the foot.

1. Muscles of the shell. These consist of two cylindrical bundles of fibres, which pass transversely from one valve to the other at points near their anterior and posterior extremities, and much nearer to their dorsal than to their ventral margins (fig. 1, *a a* and *p a.*) By their active contraction, awakened by the stimulus of nerve force, they overcome the passive antagonism of the ligament and cartilage of the hinge, and are termed respectively, from their action upon the shell-valves, anterior and posterior "adductors."

Some bivalves, e.g. the oyster, scallop (*Pecten*), and clam (*Tridacna*), have but one adductor, and were consequently

* *Forms of Animal Life.* Oxford: 1870.

placed by Lamarck in a division by themselves, under the name of *Monomyaria*, to distinguish them from the *Dimyaria*, or bivalves with two adductors. This single adductor answers to, or is "homologous" with, the posterior of the two adductors of *Anodonta.**

2. The foot, which may be regarded as a muscular mass, hollowed out for the lodgment of the digestive and reproductive organs, possesses a distinct set of muscles for its protrusion and withdrawal. The protrusion is effected by a fan-shaped muscle —the "protractor pedis"—whose fibres radiate to either side of the foot mass from a point in the valve situate to the ventral side of the impression of the anterior adductor (fig. 1, *p p*). There are two pairs of retractors. The anterior pair converge on either side from the edge and anterior portion of the foot to the dorsal portion of the side of the anterior adductor. The posterior retractors are two muscular stems which pass down on either side from the posterior portion of the foot, nearly parallel with the dorsal margin of the shell, to be inserted in the valve at the anterior and more dorsal portion of the impression of the posterior adductor (fig. 1, *r p p*). A few retractor fibres also arise a little in advance of the umbo on either side, and pass to the dorsal region of the foot (fig. 1, *r p a*).

If a small piece of one of the adductors be teazed out with needles on a glass slide, and be then placed under a ¼-inch power, it will be seen to be made up of *unstriped* fibrils, in which nuclei appear to be brought into view, after treatment with acetic acid.

In the fresh-water *Dreissena*, as well as in the marine mussel (*Mytilus edulis*), the foot is very rudimentary, being mainly subservient to the secretion of that peculiar bundle of threads, the "byssus," by which the animal anchors itself to various bodies.† It is a curious fact that the *Dreissena* has been seen to break the threads by which it was attached, and creep about by stretching and shortening its foot, somewhat in the manner of a leech. The left valve of the specimen from which fig. 1 was drawn had a byssus of this mussel attached to its exterior, which may have become detached from the animal, in the manner above described, during life.

* It is a curious fact that a species of fresh-water mussel (*Mülleria*) found in the neighbourhood of Bogota, S. America, is locomotive and di-myary when young, though fixed and mono-myary when adult.

† That greatest of naturalists, as well as of moral philosophers, Aristotle, was well acquainted with this structure as existing in the *Pinna* of the Mediterranean. He says (*De Anim. Hist.* lib. v.), Αἱ δὲ πίνναι ὀρθαὶ φύονται ἐκ τοῦ βυσσοῦ ἐν τοῖς ἀμμώδεσι καὶ βορβορώδεσιν.

The Mantle.—This organ, upon whose presence the very existence of the shell depends, completely enshrouds the rest of the animal. It is made up of two equal lateral lobes, which are adherent along their dorsal edge—that nearest the hinge—being perfectly separate, or "free," along their anterior, posterior, and ventral edges. In bivalves provided with siphons, e.g., the "gaper" (*Mya*) and the "piddock" (*Pholas*), they are adherent along all their edges, with the exception only of a small extent of the ventral margin, where they are separate to allow of the protrusion of the foot.

Into the space, or pallial chamber, included between the lobes of the mantle, project the foot, tentacles, and gills. Owing to the junction of the inner leaflets of the gill of either side, for about the extent of their posterior half, a kind of partition is formed, which fences off two chambers of unequal size. The larger and more ventrally placed may be termed the "branchial" chamber; its entrance is guarded by the fringed posterior edges of the mantle-lobes, which, therefore, may be considered as rudimentary homologues of the "inhalant" siphon in *Mya*, *Pholas*, and others. Into the smaller or dorsal chamber opens the anus; its orifice, therefore, which transmits fæcal matter, and water which has been robbed by the gills of its dissolved air, corresponds to that of the "exhalant" siphon in the bivalves just mentioned. This chamber may be conveniently called "anal."

The inner surface of the mantle, or that which looks into the pallial chamber, is lined with ciliated epithelial cells, while the cells which cover the outer surface, or that which secretes the nacre and the prismatic layer of the shell, are of the non-ciliated variety.

Digestive System.—The mouth (fig. 1, *m*), which is not provided with any organ for the mastication of food, is a somewhat triangular opening situated in advance of the foot, and just behind the anterior adductor muscle. Its lips are guarded by a pair of leaf-shaped appendages, the labial tentacles (fig. 1, *r t*, *l t*), each made up of two leaflets, joined together at their dorsal edges, and having their inner, or apposed, surfaces marked for the greater part of their length with transverse ridges. The inner leaflets by their junction form the posterior, and the outer, the anterior boundary of the mouth. The gullet, the lining membrane of which is marked by longitudinal ridges, leads soon into a stomach, somewhat irregular in shape, and communicating on its right side with a blind sac (figs. 2 and 3, *s t*), or "cæcum," which lodges a peculiar body—the "crystalline style" ("Krystallstiel," Meckel). This organ, the nature and function of which is unknown, varies very much, but is "found," according to Professor Rolleston, "in greatest

size and constancy after winter."* The pyloric portion of the stomach, or that which communicates with the intestine, is on the left side; and the first curve of the digestive tract, in that its concavity looks towards the ganglion of the foot, is termed a "neural" flexure (Huxley). The subsequent course of the intestine will be better understood by reference to figs. 2 and 3, which are diagrammatic drawings from actual specimens, than by reading a necessarily elaborate and possibly obscure description. The lining membrane of the greater part of the intestine is thrown into large and deep transverse folds; while upon the wall of the nearly terminal portion which runs beneath and parallel to the first portion of intestine, a longitudinal ridge begins to be developed, which is continued to the end of the intestine. That part of the intestine which, by virtue of its straight course, may be truly as well as analogically termed "rectum," after running through the single ventricle of the heart† (see figs. 2, 3, 4, and 6), passes over the posterior adductor, to terminate in the posterior part of the upper of the two pallial chambers.

In *Anodonta*, as in all Lamellibranchs, salivary glands are absent. There is, however, a well-developed liver, of a pale green colour and spongy texture, surrounding the stomach, into which it discharges its secretion by numerous ducts, the orifices of which may be plainly seen.

The river-mussel can scarcely be said to select food. Any matters, whether animal, vegetable, or inorganic, with which the water entering the branchial chamber may be charged, are, through the agency of cilia lining the mouth and gills,

* Various have been the theories respecting the supposed function of this body. According to some, it assists, like the sand or stones taken into a bird's gizzard, in grinding the food; others hold that it aids the foot in its resilience after contraction; while, according to Poli, it probably regulates the flow of bile into the stomach, by means of processes which project, valve-like, from its upper end into the orifices of the biliary ducts. I have recently found in a specimen of *Anodonta* what I take to be the upper portion of this organ, in the shape of a small irregular body, gelatinous in appearance and substance, and which, as Poli says, "dum arescit est maximè friabilis." Under the microscope it did not present any cellular structure, but was in appearance granular and somewhat like roughened ice. Some years ago I found this body in a *Pholas*. It was, according to my notes, "cylindrical, cartilaginous in appearance," and was, as far as I can remember, about ½ inch in length, and of the diameter of a stick of nitrate of silver. "Ejus substantia," says Poli, "refert adamussim cristallum purissimum, *flint-glass* ab Anglis nuncupatum; est autem flexilis, et valdè elasticus." This body is said to exist in most Dimyaria, but to be absent (with one exception) in the Monomyaria.

† This happens with all Lamellibranchs, with the exception of the oyster (*Ostrea*), the ship-worm (*Teredo*), the "poulette" (*Anomia*), and *Arca*.

carried, moulded into threads by a viscid secretion, along a kind of groove formed by the free edges of the latter organs, to the mouth, into which they are guided by the tentacles.

Decomposed animal and vegetable matter forms the proper food of these molluscs; and "in ponds," says Dr. Gray, "where there is plenty of food (and a dead dog, or cat, or fish affords abundance of such materials), and where the water is nearly stagnant and seldom disturbed, they become of a large size."

Circulatory System.—The anatomy and physiology of the organs of circulation is somewhat complicated, and has occupied the attention of numerous observers. The centre of the circulation, the heart (fig. 4, *h*) lies at about the middle of the dorsal region of the body, immediately beneath the junction of the mantle-lobes. It consists of a single muscular ventricle (fig. 6, *v*), through which runs the terminal portion of the intestine (see figs. 2 and 3, and fig. 6, *i*); and on the floor of which are two orifices, each of which communicates on either side with a triangular membranous auricle (fig. 6, *a u*), and is, moreover, guarded by an eyelid-like valve to prevent reflux of blood. A large pericardial sac (figs. 4 and 6, *p*) surrounds the whole. From the anterior end of the ventricle is given off an aorta, which at about the point of emergence of the intestine from the foot mass takes a direction inclining to the right, and keeps for some distance not far from the dorsal surface of the body.

Immediately beneath the pericardium is situated an organ, called, after its celebrated discoverer, the "organ of Bojanus," (figs. 1, 2, and 3, *r*). It is made up of two symmetrical lateral factors, between which lies a venous sinus (fig. 5, *v s*), each further consisting of two sacs which lie one above the other. The upper, or "pleural sac" (figs. 4, 5, 6, *p s*), roofed in by the floor of the pericardium, and communicating with its fellow for the anterior third of its extent, has smooth walls, while the walls of the lower, or "glandular sac" (figs. 4 and 6, *g s*), which is of a dark brown colour, are thrown into numerous large glandular folds. Anteriorly, each pleural sac communicates with the branchial chamber by an orifice (figs. 4 and 5, *x*), while posteriorly it is brought into relation with the glandular sac by a large fissure (figs. 4 and 5, *y*), which is somewhat masked by certain of the folds just described. The glandular sac, moreover, of either side communicates anteriorly with the pericardial space by an orifice (fig. 4, *z*), guarded by a valve which only allows of the easy passage of fluid from the former into the latter cavity.

The circulation in the river-mussel and its allies is, according to Milne-Edwards, lacunar; that is, the blood-vessels, as they recede from the heart, gradually lose their walls, and

the blood flows in channels hollowed out in various parts of the body. The researches, on the other hand, of Prof. Rolleston and Mr. Robertson, which appear to be very reliable, in that they consist mainly of careful injections, tend to prove that the blood-vascular system is a closed one. "The blood-vessels," they say, "seem to us to constitute a system of tubes closed, save at one point and at one lacuna. That point and that lacuna is the pericardial space—a cavity into which, besides the blood of the animal, the water in which it lives also finds its way." The normal course of the blood is therefore as follows: it flows through the aorta into a true capillary system, thence into the systemic veins which terminate at a sinus lodged within the foot, which in turn communicates by an orifice with a second sinus already described as lying between the two lateral factors of the organ of Bojanus; it then traverses "what may be called the renal-portal system of the organ of Bojanus;" and, lastly, reaches the gills, there to be aerated, and thence to return, laden with oxygen, to the heart, by way of the branchial veins and auricle of either side.

The external orifice of the pleural sac of either side, naturally patent in *Unio margaritifer*, cannot be well seen in *Anodonta* without separating the two laminæ of the inner gill, when it will be found situated not far behind their anterior extremity. Just on the inner side of this orifice is another opening (fig. 4, *o v*), leading into a canal, the direction of which is such, that a bristle passed into it will lie at right angles to one introduced into the pleural sac. This canal is continued into a system of tubes which ramify in the substance of the foot, and to which blind sacs, or cæca, lodging the generative glandular elements, are appended laterally. This is the aquiferous or "water-vascular" system; and its orifice just described acts as an outlet, as well for the product of the generative glands as for the fluids which course through its channels. This system is principally concerned in the great distension, preliminary to locomotion, of which the foot of the animal is capable, and which appears to be effected in this wise. When the shell gapes, the pericardial sac is necessarily dilated, and into the partial vacuum thus caused water from without passes, after first traversing the pleural and glandular sacs of the organ of Bojanus, and then makes its way into the systemic veins, whence it appears "to transude into the system of water-tubes everywhere in apposition with them."

The "organ of Bojanus" was considered by its discoverer to be the sole respiratory organ, the gills being regarded by him as only receptacles for the ova, whence he termed them "Bruthälter." It is now generally held to be a renal organ, or kidney, since the folds of its glandular sac are lined with cells, each

of which contains a dark nucleus giving the reaction of uric acid, the salts of which not unfrequently form concretions, or calculi, in the organ. M. Lacaze-Duthiers, however, though not denying that part of the function of this organ may be renal, considered that it has also some intimate connection with the state of the generative gland; and has, accordingly, termed it "annexe de la génération."

The breathing organs consist of a pair of light brown gills, or "branchiæ," on either side (fig. 1, *r g*, *l g*), which may be regarded as modified inner folds of the mantle (fig. 6, *o g*, *i g*, M). Each gill further consists of two leaves, or laminæ, separate at the attached extremity of the organ, but adherent along its free edge, thus enclosing a kind of sac, across which transverse bars run at intervals from one wall to the other. The innermost laminæ of the right and left inner gill, instead of being confined, as are the rest of the laminæ on either side, to a line of attachment just outside of the organ of Bojanus, meet together in the middle line posteriorly to the foot, and so form a kind of partition which fences off the branchial from the anal chamber. As each gill cavity is divided into a series of longitudinal canals by the transverse septa, so is a system of transverse pores formed by the intervals between the approximation of neighbouring nodulous thickenings of certain bars which may be seen, even with the naked eye, running from before backwards along the gills. Running at definite intervals across the outer side of a gill-lamina are certain lines which indicate the course of vessels returning the purified blood heart-wards, while within the gill-sac are visible other transverse lines of vessels, running in the intervals between the lines first described; these bring impure blood to the gills from the so-called "renal-portal" venous plexus of the organ of Bojanus. These efferent and afferent trunks form by inosculation a mesh-work bounding the pores and channels of the gill-sac, which are, as well as the free edge of the organ, richly fringed with cilia.

Generative System.—The sexes, as Leuwenhoeck was the first to discover, are distinct in the river-mussel and the rest of the Lamellibranchs. The generative glands, which are packed away in the foot among the other viscera, occupy, as before stated, blind sacs opening laterally into the twigs of the water-vascular tree. There is no sexual congress; but the male secretion, a milky fluid full of spermatozoa, after being discharged into the surrounding water, is drawn in by chance with the current which enters the branchial chamber of the female, and so eventually reaches the ova. In the summer the ova, which are globular and transparent, and have a firm shell, are extruded from the generative orifice (fig. 4, *o v*), and conse-

quently pass into the anterior part of the sac formed by the laminæ of the inner gill. They then travel along the dorsal region of this sac till they reach the "cloaca," or terminal part of the anal chamber; and thence, the shell being closed, and being themselves crowded on *a tergo*, of necessity pass into the external gill-sac, where, as in a kind of marsupial pouch, they pass through the rest of their period of incubation. Here the young, which were described by Rathke as parasites, under the name of *Glochidium*, are hatched, and attach themselves by means of a provisional byssus. The following points in the development of the embryo may be not uninteresting.

The yolk-mass divides into two portions, in each of which is developed a distinct mouth, intestine, and heart; these are ultimately fused together, and thus it comes that the originally double rectum is included within a single ventricle formed by the blending of once separate halves.

At a certain period, a rotation—about seven or eight times in a minute—can be seen of the embryo in the ovum. This may be due either to ciliary action, or as Dr. Ransom has shown to be the case with the ova of osseous fishes, to the influence of oxygen in the surrounding medium.

Nerve System.—There are three pairs of nerve centres, or "ganglia" (distinguishable without much difficulty by being coloured with red and yellow pigment), which are brought into relation with each other by connecting or "commissural" nerve cords, and themselves also give off nerves, which may be conveniently termed "peripheral," to various organs.

The most anterior pair of ganglia—the "labial" (fig. 7, *l g*) —are situated, one on either side of the mouth, upon the tendon of the anterior retractor muscle; the intermediate pair —the "pedal"—(fig. 7, *p g*) are closely approximated and lodged in the foot mass about half way down, and not far from its ventral margin, while the most posterior pair—the branchial or "parieto-splanchnic" (fig. 7, *b g*)—which are fused together, but are more or less bilobed, rest upon the ventral surface of the posterior adductor, and may be easily seen after cutting through the line of junction of the two inner gill laminæ.

The nerves are as follows:—*a. Commissural.* A cord passing above the mouth from one labial ganglion to the other; next, a pair of nerves to the pedal ganglion from the labial ganglion of either side; and, lastly, a pair which run straight backward through the glandular portion of the organ of Bojanus on either side to the parieto-splanchnic ganglion.—*β. Peripheral.* From the labial ganglion of either side is given off a branch anteriorly to the anterior adductor, and laterally to the anterior region of the mantle-lobe; the pedal ganglion, as its name implies, sends nerves to the foot, and also to the

organ of hearing; while from the parieto-splanchnic ganglion of either side runs, posteriorly, a branch to the rudimentary siphon, anteriorly, a cord to the gills, and, laterally, a branch to the hinder region of the mantle-lobe. In Lamellibranchs with well-developed siphons—e.g. *Pholas*—accessory siphonal ganglia are present, which are joined by a commissure to either half of the bifid branchial ganglion.

Although the Lamellibranchs, including the river-mussel, are well endowed with *general* sensibility, only one organ of "special sense" has yet been discovered in them, namely, that of hearing. It consists of a globular sac lined with ciliated epithelium, and containing a fluid in which floats a single round otolith * composed of carbonate of lime. The vibrations of this otolith, which are very easily excited, are transmitted to a nerve sent to the sac from the pedal ganglion. All organs of hearing, even the most specialised, such as that of the highest vertebrata, seem to require as one of their constituents, a sac containing otoliths floating in fluid; such is represented in ourselves by the "utricle" of the membranous labyrinth of the internal ear, where hexagonal crystals of carbonate of lime may be found floating in the "endolymph."

Although no special organ of sight has yet been discovered in *Anodonta* like unto the eyes which, "smaragdino colore coruscantes," as Poli says, stud the edge of the mantle in the scallop, this animal is undoubtedly sensitive to light. An American naturalist, from observations carried on in the rivers of central Iowa, states that Unios take cognisance, by suddenly closing their valves, of both admission and cutting off of light; and that it is, moreover, to the light rays that they are sensitive, for the same action will take place after the heat rays have been previously intercepted.

It is not improbable, though it yet remains to be proved, that the appendages which fringe the orifice of the rudimentary inhalant siphon (fig. 1, *s*) of *Anodonta* may each turn out to be a simple form of eye; this being supported by the fact that in *Pecten* the whole rim of the mantle is brought into view by the gaping of the shell, while in the river-mussel it is only the hinder margin of this organ which is ordinarily exposed and upturned to light.

A few remarks on the homologies of Lamellibranchs may not be amiss.

The shell of *Anodonta* is homologous with the "test" of the

* Anglicè—"ear-stone," from οὖς, ὠτός,=ear, and λίθος=stone. When only one auditory sac is developed, as is sometimes the case, it is said to contain, by way of compensation, as it were, two otoliths. In the Gasteropoda the otoliths are numerous, but are crowded together in one central mass.

Tunicata, e.g. the "sea-squirts" (see *Popular Science Review*, July 1869), while the mantle answers to the muscular "inner tunic" (Hancock) of the latter, the dorsal junction of its lateral lobes corresponding with the line of the Ascidian "endostyle." Professor Allman, moreover, considers that two fleshy plates, which at an early stage in the life of certain Polyzoa envelope the embryo, answer to the right and left mantle-lobes of a Lamellibranch; an indication of a relationship rather with these molluscs than with the molluscoid "lamp-shells" (*Brachiopoda*). In further support of this affinity, it is stated that the labial tentacles of the river-mussel, which some consider the homologues of the "arms" of a Brachiopod, answer to the "lophophore" of the Polyzoa. The foot of *Anodonta* is probably a modified representative of the "metapodium," or most posterior of the three divisions of the foot in the snails and slugs (*Gasteropoda*). The branchial chamber of the river-mussel according to some, according to others, the pharynx, is homologous with the ciliated gill-sac of the Ascidian. The glandular portion of the organ of Bojanus was considered by Van der Hoeven to be the homologue of the appendages of the branchial veins in the *Cephalopoda*, e.g. the cuttle-fish. The labial ganglia of the river-mussel, with their commissural cords, answer to the supra-œsophageal nerve mass in the *Odontophora*, e.g. the snail; while the parieto-splanchnic ganglion is the homologue of the single nerve centre of the Tunicata.

As Mr. Herbert Spencer has well pointed out in his *Principles of Biology*, among the Lamellibranchs "we have diverse forms accompanying diverse modes of life." Those which frequently move about, as the river-mussel, and those, such as the sea-mussel, which, though fixed, have the two shell-valves similarly conditioned, retain the bilateral symmetry characteristic of the order. In those, on the other hand, such as the oyster, where one valve is always undermost, or attached, and the other is uppermost, or most under the influence of its "environment"—the water—we find, as we ought to expect, a want of symmetry. There is further an absence of definition, or rather a great variation, of outline in the contained animals, which may be accounted for by the mutual interference to which they are subject from their clustering together; the "struggle for existence" being carried on in a very limited area, and one from which those worsted in the combat are unable, leaving their more successful rivals in possession of the field, to stir abroad in search for "pastures new."

EXPLANATION OF PLATE.

FIG. 1. From a drawing made from a specimen of *Anodonta Cygnea* dissected by the author.

The valves of the shell have been separated wide apart, in order to display the contained animal and certain markings on the interior of the shell. This can be done with moderate ease, by passing the handle of a scalpel between the mantle and one of the valves, and afterwards, with the blade of the instrument, gently cutting through the two adductor muscles close to their attachment to the valve (*a a'* and *p a'*). By bending back the freed valve with moderate force, the resistance of the hinge can be overcome without any damage resulting. All further examination of the animal ought to be made under water or dilute spirit.

It has been attempted in this figure to show as much as possible, with the least possible disturbance of parts. The animal is left lying *in situ*, resting on its right side, in the right valve of the shell. The right mantle-lobe (M) is quite undisturbed, except slightly at the anterior end. The posterior half of the left lobe (M') has been slightly drawn aside, while the anterior half has been completely turned outwards, so as to display the deeper parts of the animal, as also its own thinner portion (to which the letter M' points) which secretes the nacre, while the thicker "collar," to which the growth of the shell is due, is seen at its posterior half. The transverse prominence (*a a*) at the anterior end of the animal, where the mantle-lobes join, is caused by the underlying anterior adductor muscle, whose cut extremity is concealed by the overlapping left mantle-lobe, while that of the posterior adductor (*p a*), and of the posterior retractor of the foot (*r p p*), is seen projecting from the thin portion of the same lobe, at its posterior end. At the posterior part of the right lobe are visible the fringes which project from the inner wall of the rudimentary siphon (*s*).

Next to the mantle come the two pairs of gills (*r g, r g, l g, l g*), which are left nearly undisturbed; the outer one on the left side has, however, become somewhat contracted. Only a portion of the outer right gill is visible. The tongue-shaped body marked F is the foot; it is much contracted, and, though somewhat foreshortened in the drawing, is turned to the right, so as to show its edge and left side. The triangular opening (*m*), situated betwixt its anterior extremity and the prominence caused by the anterior adductor, is the mouth. On either side droop, like curtains, the leaf-shaped double tentacles (*r t, l t*); the inner leaflet of each joining its fellow over the root of the foot to form the posterior boundary of the mouth. Only a part of the right tentacle, the outer leaflet, is visible; that of the left tentacle is slightly turned over at the edge, so as better to bring into view

the striated inner surface of its inner leaflet. The situation of the "organ of Bojanus" is indicated by the letter *r*; its dark glandular portion appearing through the thin texture of the mantle.

On the nacreous inner surface of the left valve (V′) of the shell, rendered visible by its disengagement from the left lobe of the mantle (M′), are seen the following markings:—*a a′* and *p a′* are the impressions of the former attachments of the anterior and posterior adductor muscles respectively. From one to the other runs a curved line (*p l*) more or less parallel with the ventral edge of the shell, which is termed the "pallial impression," in that it marks the attachment of the inner boundary—not the free edge—of the "collar" of the mantle. In Lamellibranchs having retractile siphons, e.g., *Mya* and *Pholas*, a kind of bay or re-entering angle is formed between the hinder curve of the pallial impression and the scar left by the posterior adductor; this, which lodges the retractor of the siphon of its side, is called the "pallial sinus." A scar (*p p*), situated at the ventral side of that of the anterior adductor, and a few smaller markings (*r p a*) which are visible just in front of the umbo (*u*), indicate the former attachment of the protractor and anterior retractor muscles of the foot respectively. The faint lines which run parallel with the pallial impression and concentrically with the umbo (*u*) are the lines of growth, which mark the former limits of the shell valve. As many of the characters of fossil shells are determined by the aid of certain of the impressions just described, the affinities of those shells are not easily made out which have, through lying in porous chalky beds, had all the nacreous lining dissolved out, save when a cast of the interior has been formed before this came about. The letter *h* points to the inner of the two ridges bounding the groove in which the "cartilage' or spring (*c*) of the hinge is lodged.

FIGS. 2 and 3 are diagrammatic drawings from two injected preparations in the Oxford University Museum. A vertical longitudinal section has been made through the foot mass (F), in order to display the course of the digestive tube, which is seen both on its right (fig. 2) and left (fig. 3) aspects. *a a* and *p a*, anterior and posterior adductors in section; *r p p*, retractor pedis posterior, divided close to its insertion; *s t* points to the position of the "crystalline style"; *v h*, ventricle of the heart seen in section; *r*, "organ of Bojanus;" *r g* and *l g*, right and left gills. Where the intestinal tract swerves—in the first instance (fig. 2) to the left, in the second (fig. 3) to the right side—a bristle may be seen passed into the diverging portion, the course of which is further indicated in the figures by dotted lines.

FIGS. 4 and 5 illustrate the structure and relations of the "organ of Bojanus."

Fig. 4 is a semi-diagrammatic section ("coupe un peu théorique"), after Lacaze-Duthiers, of the organ in *Unio pictorum* (*Annales des Sciences Naturelles*, 4th ser.; *Zoologie*, tome iv.

Pl. V. fig. 2). It is seen from the right side. At *o v* is the orifice of the generative and aquiferous systems, and at *x* that which leads into the pleural sac ("poche périphérique"), *p s*. At the posterior part of this sac is seen the slit *y*, by which it communicates with the glandular sac ("poche centrale"), *g s*. Anteriorly, at *z*, is the opening by which the latter sac in its turn communicates with the pericardium, *p*, which invests the ventricle of the heart, *h*, and that portion of the intestine which passes through it. At *p a* the posterior adductor is represented in section, and in front of it are seen the *culs de sac*, which are formed by the reflection of the "organ of Bojanus" over it dorsally and ventrally.

Fig. 5 is a kind of ground plan of the "organ of Bojanus." At the anterior part is seen the wide communication, wanting in *Unio margaritifer*, between the pleural sac of either side, *p s*, *p s*, and its fellow. Between the sacs lies the venous sinus, *v s*. The rest of the letters have the same signification as they have in fig. 4.

FIG. 6 is a modification of a figure (13) in Professor Huxley's *Elements of Comparative Anatomy*. It is a diagrammatic representation of a vertical transverse section through an Anodon: M M, Mantle-lobes; *o g*, *i g*, outer and inner gills respectively; *v*, ventricle; and *a v*, auricle of the heart; *i*, intestine in section. The remaining letters have the same meaning as they have in previous figures.

FIG. 7 is a plan of the nerve system of the river-mussel. At *l g* is seen the left labial ganglion, which is joined to its fellow by a nerve cord. From each labial ganglion are seen to pass, in addition, the following nerves:—A branch running straight forwards to the anterior adductor, and another backwards to join the bilobed branchial, or "parieto-splanchnic" ganglion, *b g*; a cord passing down vertically to the pedal ganglion *p g*, while another runs outward to ramify over the anterior portion of the mantle-lobe.

From the pedal ganglion are given off branches—only a few of which are represented in the figure—to the auditory organs and foot; from the parieto-splanchnic ganglion passes backward on either side a nerve to the rudimentary siphon, while outwards run branches to the gills and posterior part of the mantle lobe.

The line of dots indicates a communication, wanting in *Anodonta*, but present in the marine mussel (*Mytilus*), between the anterior and posterior pallial nerves.

Owing to want of space, only part of the pallial nerves on the right side has been figured. Their course is, however, similar to that seen on the left side.

ON A SIMPLE DECIMAL SYSTEM FOR ENGLAND.

By ROYSTON-PIGOTT, M.A., M.D., Cantab., M.R.C.P., F.C.P.S., F.R.A.S., late Fellow of St. Pet. Coll. Cambridge.

IN attempting to revolutionise our national standards, we must not forget that history is apt to repeat itself. The consequences, therefore, of the arbitrary introduction of the French metrical system are not unworthy of attention.

In 1795 a provisional measure of the ten-millionth part of the earth's quadrant, reaching from the equator to the pole, and embracing 90 degrees of latitude, was adopted as the French unit of length.

In 1799 two parallels of latitude between Dunkirk and Barcelona having been carefully selected for measurement, the unit was fixed at

	39·37079,	
Or rather	39·37078980	English feet;
And the litre at	1·760773	English pints;
And the gramme at	15·43234874	English grains troy.

Seventeen years later the new Government standards still met with such violent opposition on the part of the great mass of the French people that, on March 12, 1812, the Government authorities, embarrassed with the intensity of the popular discontent, tardily conceded the use of the ancient "Système Usuel."

By this concession the people regained the use of

The toise.
The French foot.
The inch, the $\frac{1}{12}$ of the foot.
The line, " " inch.
And the point, " " line.

The double boisseau, boisseau, the half, quarter, third, sixth, eighth, and twelfth of a boisseau. The pound, or livre, of 16 onces, each once containing 8 gros, and each gros 72 grains.

How could it be possible for the illiterate classes to master

the jargon of the metric system, compounded of Latin and Greek phraseology, in a single generation? In 1840, however, the system was again enforced by a strong Government. Nearly fifty years, or nearly two generations, had passed away before the nation could be made to learn the unwelcome task imposed upon it.

The common people have a most natural and intuitive habit of reckoning by halves, quarters, thirds, eighths, &c. &c., and they go on subdividing, and then adding, particularly parts, with a surprising acuteness and accuracy. The decimal system rendered mental arithmetic at first impossible.

Our own yard admits of no less than six modes of division, without breaking the component inch.

Even to this day the French line and inch are still in use. Nobert's celebrated test lines are ruled in bands $\frac{1}{1000}$ of the Paris line in breadth. And many scientific instruments are still constructed by this obsolete measure. It is the twelfth part of the Paris inch, and too convenient to be entirely discarded.

The experience of the greater part of a century is in favour of units of measure and weight, easily divided into integral parts, rather than decimals. No whole number can be divided by three, six, or seven, unless it is a multiple of one of them, without an array of decimal figures *accurately* interminable, and to the poor man incomprehensible. It may safely be asserted that a *plébiscite* of Englishmen would give the vote in favour of retention and adaptation of our own time-honoured standards, rather than for abolition. This is an affair which pointedly

"Comes home to every man's business and bosom."

There is another consideration which has some weight, as against abolition of the standards. Almost every kind of special calculation, in particular departments of commerce or science, is wonderfully abbreviated by the principle of *common factors*. Owing to the frequency with which either 2, 3, or 5, or multiples of them, form bases of calculation, proportions or ratios innumerable have been gradually worked out, saving an enormous amount of labour.

If the reader be asked how many feet per second is traversed by a racehorse going at the rate of 30 miles per hour, an array of figures will be employed to ascertain the result. But if he be informed that 22 feet per second is exactly 15 miles per hour, he will at once perceive that 30 miles per hour is 44 feet per second. A few similar instances may be adduced.

A square mile of country receives 27 inches deep of rain per annum: this is very nearly 2 cubic feet per second.

Interest.—Since 73 is a factor in 365 days, five per cent. interest on 1,000*l.* is exactly 10*l.* in 73 days: 1*l.* for 100*l.*

Levelling.—Square of miles distance bears the same proportion to the inches difference of level (owing to earth's curvature) that 58 bears to 9.*

In surveying, 22 yards wide for a railway or road absorbs 8 acres per mile.

It is unnecessary to quote further instances to illustrate the use of common factors in units of measure.

Englishmen are attached to their British inch, pound, and gallon; and the evidence for abolishing them has yet to be produced. In the French markets, one still hears the prices asked in *sous*, *francs*, *and demi-francs*, and occasionally in centimes, and certainly no subdivision of these less than fives. In the same way, one may predict that our people will cling to the pound, shilling, and pence, the foot, yard, and acre, and the pint, gallon, and pipe, long after mètres and litres may have gained a footing here.

The arguments used by ultra-decimalists occasionally, rather weaken their cause. At the conference of the International Decimal Association, one of the principal speakers declared, that "the poor man who went to market for one pound and three-quarters of mutton, at sevenpence three-farthings a pound, would be sure to be cheated by the butcher." Let us present to this unfortunate victim of rapacity a picture of the new safeguard for his protection (which of course could be easily explained to the poor man)—

$7\frac{3}{4}$ lbs. mutton	7·75
at $1\frac{3}{4}d.$ per lb.	1·75
	3875
	5425
	775
pence . . .	13 | 5625
	4
farthings .	2 | 2500
	4
$\frac{1}{4}$ farthing .	1 | 0000

The correct price which John should pay would therefore be 13 pence 2 farthings and one-quarter. John, however, would probably think (upon consulting the talisman of his forelock) that his own way (presumptuous man) was "far away" the best. He would reckon thus: two pounds would cost $15\frac{1}{2}d.$, a quarter

* A very heavy deposit at a bank was made recently of 500*l.* against proving the curvature of water; and this sum was actually forfeited as an absurd example of ignorance of the effect of gravity. Had the following beautiful ratio (depending upon common factors) been known, "the square of the minute's dip of horizon at sea is $\frac{9}{10}$ of the feet altitude of the observer," the bettor would have been 500*l.* better off.

of a lb. twopence nearly, and, taking this from 15½*d.*, he would pay 13½*d.*, and probably the butcher would have the worst of the quarter-farthing bargain. This triumphant exposition of the advantage of the decimal system for marketing would doubtless gain many adherents to the good cause.

America is satisfied with a coinage which is seldom used or seen—the half cent, the 100th part of the dollar of four and twopence; and the English, except for very accurate accounts, are content with the farthing as the lowest coin, as the tenth part of a penny is totally unnecessary for common use.

The value of units of measure which contain factors of some one or more of the integers 2, 3, 5, 7, may be considered of sufficient importance for retaining as far as possible our national standards. Transition and accommodation, rehabilitation and revision, appear far more desirable than abolition and destruction. With this view, the problem of forming a decimal system (the advantages of which in certain cases are generally acknowledged) is entangled with difficulties of no ordinary kind; so much so, that no surprise should be felt at the lapse of fifteen years already during the consideration of this great question. This question lies, intrinsically, between destruction and adaptation. To the latter the writer gives his adhesion alone. Adopt, with the ultra-decimalists, the metric system full and complete, and a dead-lock would be given for a time to every counter in the country, to every shop and every barn. A gradual transition can alone avoid a concussion between scientific scales and the old standards. On the other hand, if new scales can be arranged, admitting easy transformation and an optional and gradual transition, practical superiority would quickly establish the new system.

Commencing with the gold standard, at present, the sovereign (containing 22 carats pure gold *) is at sixes and sevens with all foreign coin. Two courses are open, either to depress its value to 25 francs, or raise the value of the franc, so as to make 25 francs equal to it in value. Whichever course is ultimately adopted, the conversion of English money into French will no longer require arithmetic beyond *multiplication or division by* 4. But even supposing the sovereign remains worth 25 francs 20 centimes, the following method is so extremely simple as to be worthy of insertion here as a national question.

Regard 25·20, without the decimal point, as 2520; subtract from it a third of a seventh—

$$\begin{array}{r} 7)\underline{2520} \\ 3)\underline{360} \\ 120 \end{array}$$

* 22 out of 24; 18 carats standard means 18 pure out of 24 parts.

That is—subtract 120 from 2520. The remainder, after cutting off one figure, is 240 pence, or 1*l.*, the value of 25·20 francs.

```
So        7)680·40 francs.
           3)9720
             3240
Pence 12)6480 0 (cutting off one place).
              540 shillings.
               27 pounds sterling.
```

Contrariwise, the conversion of pence into francs and centimes, at the exchange of 25·20, is still more simple. All that is requisite is to add a twentieth part, after adding zero, and cut off two places. Thus—

```
         64800 pence.
Add 1/20  3240
         680·40 francs.
```

The only trouble really consists in reducing the English money into pence, as a starting point. By this very simple method, conversion of the one coinage into the value of the other renders the question of radical change comparatively indifferent, *unless it can be guaranteed that the rate of exchange between France and England shall be invariable.*

The method may be expressed in a very few words. At an invariable rate of 25·20 francs for 240 pence, francs may be converted into pence by subtracting $\frac{1}{21}$ of the francs, disregarding the decimal point, and cutting off one figure to the right; and pence be converted into francs by adding a cypher and $\frac{1}{20}$, and then cutting off two places for the centimes.

Supposing, however, Parliament consent to reducing the standard value of the sovereign nearly two pence in the pound, to the value of twenty-five francs, the whole difficulty of a decimal notation would be surmounted by the introduction of *a new coin of the value of ten farthings*, to be called a *stiver*, *silver penny*, *sil*, or *groat*, as advisable; the sovereign being divided into 1,000 farthings, instead of 960.

NEW MONEY TABLE.

£		Florins		Stivers		Farthings
1	=	10	=	100	=	1,000
		1	=	10	=	100
				1	=	10
				1 penny	=	4

English money would then acquire a powerful and perspicuous notation of singular simplicity in application, bring-

ing the franc at once into harmony with the stiver and farthing. Thus—

25 francs = £1 = 100 stivers = 1,000 farthings.
1 franc = 4 stivers = 40 farthings.
100 francs = 400 stivers = £4.

And the franc would admit of halving and quartering, and again subdividing into eight parts, twenty parts, or even forty parts, which would certainly very soon render it a favourite coin.

The new notation for 3,169*l.* 9 florins 3 stivers and 3 farthings would simply be

£3,169·933;

which again may be read as 3,169*l.* and 933 farthings, or 3,169*l.* 93 stivers and 3 farthings.

In this case, if the shilling be still coined, it must be half a florin, and therefore contain fifty farthings, instead of forty-eight. On this basis, £3,169·933 will be readily translated into £ *s. d.*

The fact, however, of the shilling containing a halfpenny more than twelve pence will probably cause its extinction, as well as sixpenny, threepenny, and fourpenny pieces, as they would no longer rank of the same value in reference to the pound sterling. A sixpence could not at the same time represent exactly six pennies and the fortieth part of a pound, when 250 pennies made the sovereign, instead of 240 (Appendix No. I.).

The demi-florin would properly take the place of the shilling (Appendix No. II.), which will inevitably disappear at last from the coinage, if the franc of ten pence be introduced, containing forty farthings.

DECIMAL WEIGHTS.

The exceedingly simple and elegant definition of the English pound avoirdupois, founded upon a standard gallon measure—that it is the tenth part of the weight of a gallon of distilled water, of 277·274 cubic inches, when the barometer stands at 30 inches, and the air is 62° Fahrenheit, the water being weighed at the sea-level, and that this pound weighs exactly 7,000 grains—admits of a notation of the highest possible convenience.

It is with diffidence that the writer, in the face of so much laborious and patient investigation, now carried forwards for so many years, ventures to propose the introduction of the following scheme, by the adoption of a unit of weight, to be called

the KARAT, bearing exactly the same proportion to the pound avoirdupois that the gramme bears to the kilogramme, viz. the 1,000th part. The demi-ton, or half-ton, will then contain 1,000 lbs. instead of 1,120, and under the decimal system of weights, the 12, 14, and 16 pound stones must be abolished, and one uniform stone of 10 lbs. be introduced, to complete the scale. All heavy weights could be expressed in two terms, the lowest of which would be karats, except in the case of grains.

TABLE OF DECIMAL WEIGHTS.

1 demi-ton	=	10 cwt.
1 cwt.	=	100 lbs. (= 10 stones?)
1 lb.	=	10 ounces.
1 ounce	=	10 drams.
1 dram	=	10 karats.
1 karat	=	7 grains.

Example of the power of this notation: Take 5,555,096,986·5 karats—

They denote either 5,555,096,986½ karats,
or 555,509,698 drams and 6½ karats,
or 55,550,969 ounces and 86½ karats,
or 5,555,096 lbs. and 986½ karats.

Or, read in the order of numeration, these thousands of millions of grains denote with precision and brevity 5,555 demi-tons 96 lbs. 9 ounces 8 drams 6 karats and a half (or 3½ grains).

The most complicated weights can be employed at once without *reduction*, and any arithmetical operation at once be performed upon their amount.

To bring this system into relation with the French kilogramme, the relative value of one pound avoirdupois, of 7,000 grains to the gramme, must necessarily be employed—

The kilogramme : pound as 1,000 grammes to 7,000 grains :: 1 *gramme* : 1 *karat.*

The new karat therefore bears the same proportion to the gramme as the pound does to the kilogramme—

The gramme = 7)15·43234874 grains.
or 2·20462124 karats.
The kilogramme = 2·20462124 lbs.

The KARAT is, in fact, the thousandth part of the pound, just as the gramme is $\frac{1}{1000}$ of the kilogramme. This sequence enables all weights, however various, in the new scale, to be decimally converted into kilogrammes by simple multiplication or division.

The value of complicated units of weights of all the new denominations, viz. at pounds, florins, stivers, and even farthings per demi-ton, can immediately be obtained by simple multiplication. The value expresses itself, and by multiplying it by 100 and dividing by 4, the equivalent francs are known. The extraordinary simplicity of the calculation is the greatest possible recommendation for such a choice of units of money and weight.

No matter what denomination be employed, English weights would be at once transformed into kilogrammes by multiplying them by the reciprocal of the kilogrammes—

(1 ÷ 2·20462124 =) ·453592662,

or as many places of decimals as will give the required minuteness of accuracy.

Francs can readily be converted into the new decimal coinage by multiplying it by 4 and dividing by 100—

```
      7·75 francs.                    27·75 francs.
      4                                4
100)31·00                       100)111·00
      ·31  3 florins 1 stiver.        1·100 £1 1 florin 1 stiver.

           39·55  francs.
              4
           1·5820  £1 5 florins 8 stivers and 2 farthings.
```

Conversely, pounds, florins and stivers will appear as francs and centimes simply by dividing by 4, after multiplying by 100.

The stiver, being the quarter of a franc, will tend greatly to familiarise the οἱ πολλοὶ with the decimal scale; whilst the temporary retention of the shilling, and sixpence, and threepenny piece, will still afford that ready subdivision so useful to rapid marketing.

DECIMAL MEASURES.

The regulation of measures of length and capacity is an invidious task, as involving so many deeply-rooted associations. Thus: 6,075$\frac{4}{5}$ feet are a nautical mile.

A change here will array all the sailors against us. And the log at sea, of 52 feet and a fraction, run out by the half-minute glass, ascertains the speed of the good ship at so many knots per hour. It is as vain at present to tamper with the sailor's log as with his grog.

There are at least seven or eight different measures of the wine pipe; seven kinds of stones; six kinds of barrels by measure, and many by weight. The aunes of hock, hogsheads of claret and madeira, and pipe of port, are equally obnoxious

to uniformity. Chinese money and liquids have, however, from time immemorial, been decimally reckoned—

10 kops	=	1 shing-tsong.
10 shings	=	1 tan.
10 tans	=	1 hwuh.

The nearest approach to this at present attained is the pint, containing 20 ounces, half a pint 10 ounces. But our usual liquid and dry measures know no such law.

Since the gallon weighs exactly ten pounds, it ought to be divided into ten pints, the pint into ten ounces, the ounce into ten drams, and the dram into ten KARATS. There would then be a most perfect coincidence (for distilled water) between both weight and capacity.

An ounce would mean either the tenth part of a pound or of a pint, and would really represent also 100 KARATS, of 7 grains each, of distilled water.

The dram, the tenth part of the ounce, would be 10 karats, or 70 grains. In short, the measures of capacity would exactly correspond decimally with avoirdupois weights. The ounce is already used as a measure of liquids in medicine, and one dangerous source of mistake and confusion would thus be annihilated.

Economy of labour in practical arithmetical calculations is the indefeasible principle upon which a new national metrical notation should be framed.

The writer, after some consideration, recommends to the attention of practical men the following scales of measures of capacity and length.

MEASURES OF CAPACITY.

1 pipe	=	100 gallons.
1 gallon	=	10 pints = 100 ounces = 1,000 drams.
1 pint	=	10 ounces.
1 ounce	=	10 drams.
1 dram	=	10 KARATS, of 7 *drops* or *grains each.*
1 karat	=	7 drops, of a grain each.

In this case, 1 drop or gutta would equal in weight exactly the standard grain, and the dram would contain 70 drops or grains. In medicine, the weight of a drop, as well as its magnitude, has great variety, according to the nature of the liquid and form of the lip of the dropping-vessel. (App. No. III.)

MEASURES OF LENGTH.

It may be remarked that the tenth part of a foot has already been adopted on the breadth of the British penny; and the

retention of the British foot would be hailed by nearly all artisans as a great convenience, as still preserving the inch and the yard.

The scale would thus be conveniently decimalised from a foot unit—

1 foot	=	10	bronze penny inches.
"	=	100	parts.

The division of the foot into 100 decimal parts at once gives a name to the lowest denomination as "parts," and each part will be, of course, the $\frac{12}{1000}$ of an inch. 365 feet $96\frac{1}{2}$ parts will be denoted by 365·965 feet.

Decimal "parts" will answer every purpose of reference and calculation. Acreage, mileage, whether nautical or terranean, as well as the divisions of the circle, will probably be handed over to another generation, unless the country is determined to accept the French mètre, devoid of all natural subdivisions into thirds, fourths, sixths, &c. &c.

All the complicated cross multiplication of duodecimals will be at once extinguished by the use of the foot and its decimal parts.

Ten feet might be called a rod, and a hundred feet something else; but it is doubtful whether such a measure would serve any useful purpose, except in calculation. It may be shrewdly suspected that any further upward change in the foot will inevitably lead us to adopt the French mètre whole and undivided.

APPENDIX.

The tables in this appendix have been carefully calculated and verified by reversing the process. In that for equivalents of grammes, it will be seen that 11 grammes equal $170\frac{3}{4}$ English grains, within the 6-1,000th of a grain.

In the table of English and French equivalents of measures, I have calculated them to eight places of decimals, so as to be accurate for seven at least.

It hardly seemed desirable to convert litres into pints in a tabular form, and, conversely, gallons or pints into litres.

The relation of the KARAT to the pound avoirdupois being precisely similar to that of the gramme to the kilogramme, a table has been calculated to eight places of decimals for purposes of reference.

No. I.

The facility and power of the notation may be considered in the following example:—

Example: The value of 250 tons (500 demi-tons) 5 cwt. 93 lbs. 9 ozs. 9 drms, at 58*l.* 4 florins 6 stivers 3 farthings per ton, may be ascertained by common multiplication—

500·59399
Multiplied by 58·463

Result 29266·226 or 2,966*l.* 2 fl. 2 st. 6 far.

This can be reduced into francs by multiplying by 100 and dividing by 4, if 25 francs = 1*l.*—

29266·226
100
4)2926622·6
731655·65 731,655 francs 65 centimes.

Again, if the demi-tons be reduced to lbs., by moving the decimal points three places to the right, the amount will be reduced to kilogrammes by multiplying it by the reciprocal of the number of lbs. to the kilogramme.

Thus: 5 demi-tons 0 cwt. 5 lbs. 9 ozs. 3 drams.

5·00593 = 5005·93lbs.
Multiplied by ·45359 ·45359
2270·639 kilogrammes.*

No. II.

Example: Interest on 98,865*l.* 9 florins 8 stivers 3 farthings, at $3\frac{3}{4}$ per cent., is thus quickly obtained by the new coinage proposed, and thus expressed:†

98865·983
3·75
100)370747·43625
3707·474;3625,

which denotes 3,707*l.* 4 florins 7 stivers 4 farthings, easily converted by the table of money into 3,707*l.* 9*s.* $6\frac{1}{2}$*d.*, where of necessity the shilling, being the twentieth part of 1,000 farthings, contains 12 pence and a halfpenny, conveniently to be represented by a demi-florin or five stivers.

---

* If greater accuracy be required, the multiplier should contain as many places of decimals as the result contains figures, the complete decimal to eight places being ·45359266.

† Conversely, 3,707*l.* 4 florins 7 stivers 4 farthings can thus readily be reduced into francs and centimes—

4)3707·474
926·8685
100
92686·85 *francs and centimes.*

Old-fashioned people (probably past sixty) will long adhere to their favourite sixpence and shilling, and perhaps will in time regard a shilling as the spurious half of a florin; but the sooner both shillings and sixpences, fourpennies and threepennies, cease to be coined, the sooner will the decimal money fight its way into general use.

No. III.

We may write, according to the new notation—as 1 pint = 7000 grams or 1000 karats.

960·8699 karats or 9608·699 pints;

or, if a pipe be exactly 100 gallons, this will be expressed in either pipes, gallons, pints, or karats—

$$960 \text{ gallons} = 9 \text{ pipes } 60 \text{ gallons} = \overset{\text{P.}}{9}\ \overset{\text{G.}}{60}$$

$$\overset{\text{G.}}{960\cdot8699} = \overset{\text{P}}{9\cdot608699} = 9608699 \text{ karats.}$$

Gallons or pipes are here expressed both by weight and measure, the karat being equally a measure of capacity or of weight.

Again, since

$$1 \text{ karat} = \tfrac{1}{1000} \text{ part of a pint or pound,}$$

$$12 \text{ pipes } 80 \text{ gallons } 9 \text{ pints } 0 \text{ ounce } 9 \text{ drams } 9 \text{ karats} = \overset{\text{P.}}{12\cdot809099},$$

$$\text{which, at } 120\cdot95 \text{ francs per pipe,} = 12\cdot809099 \times 120\cdot95$$

$$= 4)\overline{1549\cdot2605} \text{ francs}$$

$$\text{Divide by } 10)\overline{387\cdot315}$$

$$£38\cdot7315,$$

or 38*l.* 7 florins 3 stivers $1\frac{1}{2}$ farthing.

A remarkable instance of the power of the new notation, concise and self-interpreting, without reduction.

The great advantage of using *reciprocals** for multiplication, instead of dividing by the *constant*, is shown by the following examples:—

$$
\begin{array}{r}
99\cdot999 \text{ kilogrammes} \\
\cdot4536 \text{ (constant)} \\
\hline
599994 \\
299997 \\
499995 \\
399996 \\
\hline
45\cdot359{,}5464 \qquad 45\cdot359,
\end{array}
$$

or 45 lbs. 359 karats are equivalent to 99·999 kilogrammes. By division, five places of decimals must be used—and we get

---

* Reciprocal number is such a number or fraction or decimal as multiplied by the number makes unity. Thus $\frac{1}{2}$ is the reciprocal of two, $\frac{1}{250}$ of 250.

2·20462)99·99900(45·3588, or 45·359 lbs. nearly.
881848

1181420
1102310 — Equivalent to
·79110 — 45 lbs. 359 karats.
661386

1297140
1102310

194830
1763696

184604
1763696

82344

The answer is laboriously obtained with three times the amount of computation.

TABLE OF RELATIVE WEIGHTS AND MEASURES.

One mètre	= 39·37078980 inches.	
„	= 3·28089916 feet.	
„	= 1·09363305 yard.	
One inch	= 25·39954113 millimètres.	
One mile	= 1609·314926165447 mètres.	
One French toise	= 2 mètres.	
One French foot	= $333\frac{1}{3}$ millimètres	= 12·789 inches.
„ inch	= $27\frac{3}{4}$ „	= 1·065 „
Line = $\frac{1}{12}$ inch	= $2\frac{1}{3}$ „	= 0·088815 „
Point	= $\frac{1}{6}$ part of a line	= 0·1480 „
Jewel carat	= $3\frac{1}{6}$ grains diamond weight.	
Troy carats, stand. gold	= 22 out of 24 pure gold.	
Hamburg carat	= 12 grains.	
Par in silver, £1	= 25 francs.	
Par in gold, £1	= 25·22 „	
Weight of sovereign	= 123·27447 grains.	
Of pure gold	= 113·00159 „	
Weight of shilling	= 87·27273 „	
Of pure silver	= 80·72727 „	
The kilogramme	= 2·2046212 4 lbs.	
The litre	= cube of a decimètre	
„	= $3·937078 98^3$ cubic inches.	
„	= 61·027082 „ „	
The gallon (dist. water)	= 277·274 cubic in., weighing 70,000 grs	

DECIMAL EQUIVALENTS OF ENGLISH AND FRENCH UNITS OF MEASURES AND WEIGHTS.

British Inches	Millimètres	Millimètres	British Inches
1	25·39954113	1	·039370789
2	50·79908226	2	·078741579
3	76·19862339	3	·118112369
4	101·59816452	4	·157483159
5	126·99770566	5	·196853949
6	152·39724679	6	·236224738
7	177·79678792	7	·275595528
8	203·19633905	8	·314966318
9	223·59587018	9	·354337108
10	253·99541132	10	·393707898
12 (foot)	304·79449358	20	·787415796
20	507·99082226	40	1·574831592
30	761·98623396	50	1·968553949
36 (yard)	914·38348075	100	3·93707898
40	1015·98164528	1000 (mètre)	3·93707898
50	1269·97705566		
100	2539·95411326		

KARATS, 7 grains each	Grammes	Grammes	GRAINS
1	·453592	1	15·43234874
2	·907185	2	38·6469748
3	1·360777	3	46·29704622
4	1·814370	4	61·72939496
5	2·267963	5	77·1617437
10	4·535926	10	154·3234874
20	9·071852	11	169·75583614
50	22·679632	20	308·6469748
100	45·359264	50	771·617437
1000 (one pound avoïrdupois)	453·592662	100	1543·234874
		1000 (kilogramme)	15432·34874

WHAT FILLS THE STAR-DEPTHS?

BY RICHARD A. PROCTOR, B.A., F.R.A.S.

AUTHOR OF "SATURN AND ITS SYSTEM," "HALF-HOURS WITH THE STARS," AND "OTHER WORLDS THAN OURS."

FOR more than two centuries and a half astronomers have studied the depths of heaven with the telescope, piercing farther and yet farther into wondrous abysms of space, gathering clearer and yet clearer information as to the structure of celestial objects, and accumulating an untold wealth of knowledge respecting the habitudes of the great system whereof our sun is a constituent orb. During all this process of research, the great end and aim of astronomers has been to extend the range of their instrumental appliances, in order to analyse more scrutinisingly the features of each portion of the celestial depths. Now and then it has occurred to some among their number to endeavour to combine the results which have been gathered together with so much pains; but these attempts have been almost lost sight of amidst the continual accumulation of fresh facts. The efforts made to arrange and systematise our knowledge have been altogether out of proportion with its extent.

And, very strangely, when any attempts are made to educe from the labours of observers their proper significance, to reap the harvest which is already ripe, or rather to grind the corn which is already in our garners, the cry is raised that such attempts are fit only for the theorists, that they argue a want of appreciation of the labours of observers, and that we have more to hope from fresh observations than from any process of mere reasoning. Surprising, indeed, that those who say "Let us use the observations already made," should be accused of undervaluing observation; and that those who can find no value or significance in past observations, should call so eagerly for fresh ones!

I make these remarks because I am about to exhibit certain

views respecting the habitudes of interstellar space, which have been formed from the study of the past labours of astronomers. I am fully sensible of the fact that to many I should seem better worthy of a hearing, if I nightly timed my watch by the stars, if I had spent a few years of labour in attempting to divide well-known double stars with inadequate telescopic power, or if I had in some other equally convincing manner exhibited my title to be regarded as a member of the now large array of amateur telescopists who work so hard and effect so little, and suppose themselves to be practical astronomers. Let me not be misunderstood, however. It is only because I wish to see amateur telescopists engaged on more useful researches, because I wish to see them devote a little more consideration than they do now to the thought of advancing astronomy, that I speak slightingly of the modes in which at present they are for the most part wasting time. We want all their help, and more, to advance the interest of our well-loved science; all their telescopic appliances are too few for the work astronomers would like to see them doing.

In studying the heavens, we have always this great difficulty, that we are looking at objects which are in reality at very different distances, but which appear to lie on the concave surface of a vast spherical enclosure. It seems almost hopeless to attempt by any processes of observation to obtain reliable estimates of the distances of all, save a very few, of the fixed stars. It is not going too far to say that we are tolerably certain of the distance of only one star in the heavens—the star, Alpha Centauri. This being the case, and the heavens spangled with millions of objects at altogether unknown distances, we must look carefully round us for evidence of another kind than that derived from actual measurement—we must look for signs of association, for definite laws of aggregation—if any such exist—and, if possible, we must apply that mode of inquiry from analogy which Sir William Herschel found in many instances so effective.

And here, as I have mentioned the name of this great astronomer, to whom we owe the first systematic survey of the heavens, and the first attempt to reduce the results of observation into law and order, I wish with, extreme diffidence, to point to what I cannot but consider an error of judgment in his selection of the principles which were to guide his survey of the heavens. It appears to me, that it would have been in all respects better had his first processes of stellar observation been directed to gauge the probability that this or that law of distribution prevails in the heavens, rather than to the application of a system of star-gauging, which, if founded on a mistaken assumption, was necessarily but a waste of labour. It would

have been a misfortune if the unequalled observing qualities of either the elder or the younger Herschel had been misapplied for a single hour; but the possibility that the labours of both these astronomers should have been devoted year after year to a process which (if my views are just) was practically useless, is painful indeed to reflect upon. It is true that the labours of the Herschels have been so numerous and so widely extended, that even the recognition of their star-gaugings as of little real utility would leave the great mass of useful results credited to them almost unaffected; but it would remain none the less a misfortune that labours, which in the case of other men would have worthily filled a lifetime, should have been misdirected.

And yet, when one considers the matter apart from preconceived notions, how inconceivably small the chance appears that these laws of distribution believed in by Sir William Herschel actually prevail within the sidereal depths. How amazing that to his clear perceptions the idea should ever have seemed probable that the celestial spaces are occupied only by orbs resembling our sun. For, be it distinctly noted, that his belief in the existence of gaseous nebulæ, and orbs in various stages of development, belongs to the later part of his career as an observer. Undoubtedly the whole system of star-gauging was founded upon the belief that the sidereal system consists of stars, varying greatly perhaps in size, but still not so greatly but that the least of them would be visible in Herschel's great telescope, as far as the very limits of the sidereal system, and that these stars are distributed with a certain general uniformity throughout space.

It is well to observe how fatally any error in this fundamental hypothesis affects the significance of any system of star-gauging. We turn a telescope in a given direction, and we see, perhaps, but four or five faint stars. According to the Herschelian hypothesis, the limits of the sidereal system are near to us in that direction, because the stars seen are so few, and those stars being necessarily within those limits, and faint, belong probably to the lower orders of real magnitude. But what if that hypothesis be erroneous—if there may exist in this or that direction vast blank spaces a thousandfold larger, perhaps, than the whole sphere of the visible stars in extent? Then, perchance, these four or five faint stars may lie farther from us than the farthest belonging to some of the richer star-fields; they may form a group of orbs which individually surpass Sirius or Canopus in magnificence, and are separated from each other by distances exceeding many thousandfold those which separate our sun from neighbouring luminaries. But, yet again, suppose that in any direction our telescope reveal crowded star-fields, orbs of all orders of apparent brightness, "strewn as

by handfuls, and both hands full," and each increase of power adding fresh riches to the display. According to the Herschelian hypothesis, there is but one explanation of these wonders; we are looking into a widely extended part of the sidereal system, and those different orders of stars lie at different orders of distance—the farthest at distances so enormous that we cannot attain to them. But, in what a different light we must regard the scene if we remember the possibility that that wondrous wealth of stellar display need by no means argue enormous extension. All these sparkling orbs may be gathered into one region of space, their various orders of apparent lustre arguing various orders of real magnitude. Instead of looking into star-lit depths, which extend linearly from the eye, far out into space beyond the ordinary limits of distance separating from us the outer bounds of the sidereal system, we may in fact be contemplating a wondrously variegated star-group.

But the conclusions we are to form must be founded not on the consideration of what *may* be, but on our observation of what *is*. There is abundant evidence for forming probable views respecting the general laws prevailing within the sidereal system; at any rate, for deciding whether it is more probable that there is or not any general uniformity of distribution within its limits.

One direct consequence of the laws of probability has been very much lost sight of in dealing with the subject we are now engaged upon. It has been urged that where so many stars are spread over the heavens, at so many various distances, we ought not to be surprised if very great varieties of distribution should be observed, nor conclude, therefore, that the general uniformity predicated by Sir William Herschel may not prevail as respects distribution in space. It has been forgotten that the vastness of the numbers in question should tend to a uniformity of apparent distribution, instead of the reverse.

I had been led myself to overlook this consideration, obvious as it is, until it was impressed upon me in a very striking manner during a somewhat novel process of research.

I wished to determine what peculiarities of distribution might be expected to appear among a number of points spread over a plane surface perfectly at random. It is clear that this is a preliminary consideration very necessary for the purpose of determining whether the laws of distribution seen among the stars are accidental or not. Now, the problem of determining by purely mathematical considerations what peculiarities would probably appear in a chance distribution of any given number of points, is one which may be regarded as altogether too difficult for solution. Very simple problems of probability have been found perplexing, insomuch that two eminent ma-

thematicians of the last century are said to have disputed over the question whether the chance of tossing one head and one tail in two throws of a coin were one-half or one-third.* But problems concerning the chance distribution of points are specially difficult, as any one will find who tries a few apparently simple ones.† Therefore, I sought to solve this particularly complex problem in a practical manner, by simply spreading a number of points at random, and examining the result. But *how* to distribute points perfectly at random? It seems very easy, but is not so by any means. Suppose we take a handful of grains, and throw them upon a table. Will they then be strewn without law or order? Very far from it. The fact that they have all come from the same hand will lead to very obvious effects, taking away altogether from the desired random character of the distribution. Then, again, suppose we were to distribute grains over a table from a sieve as large in extent as the table, and uniformly filled. In this case the grains would be distributed with a uniformity not appertaining to chance distribution. And so of a number of other contrivances which may be thought of; in every case of mechanical distribution, we always find either an enforced inequality or an enforced equality of distribution, not that really random distribution which we require.

The plan I actually adopted, if laborious, was at least satisfactory in this respect. I took a table of logarithms (any other book full of tabulated figures would have done equally well), and opening the book at random, brought down the point of a pencil upon the page of figures. The numeral on which, or nearest to which, the point fell, I entered in a book. In this way I took out several thousand figures, following each other in altogether random sequence. Then, having divided two adjacent sides of a square into 100 equal parts, I drew parallels to the sides, through the points of division, thus dividing the square into 10,000 small squares. Now, suppose the first four figures in my list to have been 7324. I took the seventy-third

* The erroneous reasoning by which the answer is made to be one-third seldom fails to puzzle the uninitiated. "There are," said D'Alembert, "three possible events: either two heads must be thrown, or two tails, or head and tail; of these three possible events, only one is favourable. The chance of that event is, therefore, precisely the same as the chance of drawing one particular ball out of a bag containing three,—that is, it is one-third."

† For instance, here are two: (1.) On a square surface of given size (say one square foot) two points are marked in at random; what is the chance that they will be within a given distance (say one inch) of each other? (2.) Three bullets strike a circular target three feet in diameter; what is the chance that the lines joining the three points where the target is struck will include a triangle less than one square foot in area?

parallel measured from one side, and the twenty-fourth measured from the adjacent side of the square, and at the point where these lines intersected I placed a black dot. I treated the next four numerals in the same way; and so on, until I had exhausted the series. I thus had upwards of 1,000 dots distributed perfectly at random over the square.

Now, as I went on marking in the dots, I found that at first groups and streams might very well be imagined to exist among the dots. But, as the process continued, these groups and streams were obliterated (so to speak), until at length, when all the dots were marked in, it required a very fanciful imagination indeed to conceive that any signs of special laws of distribution existed among them. I was thus reminded of the great law of probability, that the mere numerical increase of trials ensures a steady increase in the uniformity of the results. For example, if one tosses a coin a few times, there will often result a very remarkable preponderance of "heads" or "tails;" but where one continues tossing the coin a great number of times, the ratio between the number of "heads" and "tails" approaches more and more nearly to equality. And, applying this law to the case under consideration, it follows that if a very large square sheet were divided into an indefinitely large number of small squares, and an indefinitely large number of perfectly equal dots were marked in according to my plan, or according to any plan securing a perfectly random distribution,* an accurate miniature of that sheet (taken by photography, suppose) would be found as uniformly tinted by these chance-distributed dots as by any mechanical process of uniform dotting.

Therefore, supposing that any general approach to uniformity of distribution exists among the stars, we ought to find all signs of special arrangement disappearing as we extend the range of our researches. We cannot then possibly explain the peculiarities actually observed as due to the enormous number of stars and the resulting probability that remarkable arrange-

* One of the most interesting results of any such process as that above described, is the striking evidence afforded of the fact that any circumstance affecting the random character of the distribution is sure to tell when many trials are made. I was led to enquire whether in my list of numerals any special numbers seemed unduly to preponderate. I found that the number 8 appeared oftener than the rest, and that to an extent which I could not ascribe to mere accident; 1 and 7, on the other hand, appeared less frequently than the rest. The reason is obvious: the figure 8 covers more space, 1 and 7 less space, than any other figures; so that when the point of the pencil fell between an 8 and one of these figures, the chances were more favourable to the 8 being selected as the figure nearest to which the point came.

ments might accordingly be looked for, since the exact reverse is the case.

Now I conceive that so soon as we pass the third or fourth orders of star magnitude, we reach orders large enough, numerically, to supply the information, clear of the effects of mere accident, which we actually need in this instance. Among the stars down to the fifth magnitude, there is surely a sufficient number to enable us to begin to reason, with some degree of confidence, as to the constitution of stellar space. Therefore, when, in 1866, I was constructing my gnomonic maps of the heavens, in which stars of these orders are included, I was disposed to regard the signs I met with of special laws of distribution as significant of real laws; and accordingly I put forward, in that year, the theory that the stars are aggregated into streams and clustering aggregations, with relatively bare spaces all round them. And, furthermore, it seemed to me, even at that stage of the enquiry into the habitudes of stellar space, that the Milky Way probably consists of relatively minute stars, and not, as had been supposed, of stars generally comparable with our sun, and forming a system extending to enormous distances on all sides of us; while I was led to regard the nebulæ as belonging to the sidereal system, and not as external galaxies resembling that system.

But recently I have had occasion to apply processes of mapping to stars down to the sixth magnitude, or, in all, to four times as many stars as before. And clearly one cannot regard signs of arrangement among so many as 6,000 stars as being due to accident. The largeness of the number altogether precludes the possibility of this being the case.

When, therefore, it appears that among stars of the first six magnitudes there are signs of special laws of aggregation, we are bound to accept as legitimately following from the evidence, the conclusion that real laws of aggregation exist among the stars. We may not be able to tell what these laws are—we may mistake a number of separate clusters for a stream of stars, or the nearer end of a stream for the farther end, and so on; but the broad fact remains that the stars are gathered into some regions and withdrawn from others, and, further, that within the same region of space stars of very different orders are, in many instances, gathered together.

The general results of a systematic survey of the stars of the first six magnitudes seem certainly to force upon us such conclusions. They are as follows:—

1. The southern hemisphere contains more stars of the orders considered than the northern, in the proportion of about seven to five.

2. The stars of these orders are gathered into two definite

regions—a northern and a southern—so markedly, that the distribution of stars within these regions is richer than the distribution over the rest of the heavens, in the proportion of about five to two.

3. The stars of these orders are associated in the most intimate manner with the Milky Way, insomuch that when the Milky Way is included with the two rich regions above-named, it appears that stars in the single division thus formed are distributed about three times as richly as over the remaining portion of the heavens.

These results cannot be regarded as due to mere chance-distribution, unless we are to forget all the rules which the science of probabilities lays down for our guidance in such cases.* And if once we admit that they result from real laws of aggregation, our estimate of the nature of the sidereal system is wholly altered. We see at once that we are not dealing with a system that can be gauged; for if within the limits of naked-eye vision there exist these aggregations and these *lacunæ*, we may be full sure that throughout the sidereal system they exist also, and what confidence can we have in any system of gauging applied to depths so diversely occupied? Our sounding-line may light on a rich stream or clustering aggregation of stars, or it may pass through relatively vacant spaces, yet we can by no means conclude from the richness of the one region or the poverty of the other, that the line reaches either very far off or relatively very near the limits of the sidereal scheme.

Again, regarding either of the rich regions referred to above as consisting wholly or in part of a definite aggregation of stars happening to lie relatively near to us, is it not abundantly evident that other such aggregations at different orders of distance would exhibit many of the features which we see in the nebulæ, and have been in the habit of associating with regions lying beyond the sidereal system? Or—to arrive at a similar conclusion from different evidence—if the Milky Way be

* The effect of numbers in diminishing the probability of such signs of law resulting from mere chance-distribution, must here again be insisted upon. It is most important to notice how it bears upon the conclusions we are to form. Here is a simple illustration of the law of probabilities in question.—Suppose we toss a coin 4, 8, 12, and 16 times, and inquire what is the chance that in the several cases either heads or tails will preponderate in the proportion of 3 to 1. Now the exact mathematical solution of this problem shows that when 4 trials take place, the chance is $\frac{5}{8}$, or more than $\frac{1}{2}$ when 8, the chance is $\frac{37}{128}$, or less than $\frac{1}{3}$; when 12, the chance is $\frac{299}{2048}$, or little more than $\frac{1}{7}$; while when 16 trials take place, the chance is reduced to $\frac{2517}{32768}$, or less than $\frac{1}{13}$. The chance would become indefinitely small if, instead of 16, we took several thousand trials.

really as it seems (from the third of the above results), a stream of stars of many different orders, with an enormous preponderance of relatively small stars, is it not abundantly evident that, supposing any portion of this stream removed to a greater distance, the fainter regions would vanish first, and that the brighter regions would appear as small discrete patches of nebulous light, or naked-eye nebulæ? Now conceiving the stream yet further removed, until even these patches become telescopic objects, would they not in all respects resemble the stellar but irresolvable nebulæ?

Thus it appears to me that we have not only no grounds for believing that the nebulæ are external galaxies, but tolerably distinct evidence that the stellar nebulæ are distant aggregations of stars of many orders of magnitude. Such aggregations may also themselves present, and doubtless they do present, all orders of magnitude, precisely as within the two rich stellar regions above referred to we find every conceivable variety of aggregation, and precisely also as within the Milky Way we find, on the one hand, bright regions as extensive as that which lies in the constellation Cygnus, and, on the other hand, regions as limited as the double cluster in Perseus.

Have we, on the other hand, any satisfactory reasons for regarding the nebulæ as external galaxies? Tracing back the course of that process of discussion which has led to the nebulæ being commonly so regarded, can we indicate any one argument which may be looked upon as definitely pointing to such a conclusion? *I know not of one.* I have carefully studied the writings of Sir William Herschel, and I venture to assert, without fear of valid contradiction, that every single consideration adduced in favour of the nebulæ being external galaxies has been founded on the assumption that the sidereal system is continuous—that is, on an assumption which Sir William Herschel himself was the first to throw doubt upon.

But the evidence derivable even from Sir William Herschel's own writings goes farther than this. He not only formed views respecting the sidereal system diametrically opposed to those which he had entertained when his conceptions respecting the stellar nebulæ were put forward, but he arrived, by his careful study of the nebulæ system, at a conclusion which, rightly interpreted, brings the nebulæ into close association with the sidereal system. For, noticing the aggregation of nebulæ at the northern region, which lies farthest from the Milky Way, he confidently expressed his belief that any sound theory of the universe must account for that peculiar relation. In other words, no theory of the universe can be regarded as sound which treats that relation as accidental. So that Sir William Herschel himself regarded the nebulæ system in a light which

in effect associates them in a real manner with the sidereal system.

In fact, and in conclusion, that great astronomer was not bound, as so many of his modern followers have been, by his own earlier theories. As his researches continued, his views gradually changed. The process of change went on during the whole course of his career as an observer. He advanced steadily from truth to truth, and when at length the close of his labours approached, he looked onwards, not backwards. He would have been the last to desire that astronomers should take even his latest and best theories as including all that they could desire or hope to know; but it would have been even more painful to him to imagine that the views he held, when as yet his labours were but beginning, should be adopted by future astronomers in preference to those which were the fruits of his ripened experience.

ON THE APPARATUS EMPLOYED IN DEEP-SEA EXPLORATIONS, ON BOARD H.M.S. *PORCUPINE*, IN THE SUMMER OF 1869.

By Wm. LANT CARPENTER, B.A., B.Sc., (a Member of the Expedition).

THE object of this article is not to give an account of the remarkable results obtained in the late deep-sea dredging expeditions, since an abstract of them has been given to the public in the *Proceedings of the Royal Institution*, and in other scientific journals, and will shortly appear more in detail in the *Proceedings of the Royal Society*. The aim of the present writer is to endeavour to give the readers of the *Popular Science Review* some idea of the means employed to obtain these results, by describing the apparatus used, its mode of working, the precautions taken to avoid sources of error, &c.

During the time that the expedition was under the charge of Mr. J. Gwyn Jeffreys, F.R.S., from the latter end of May to the middle of July, the writer was entrusted with the chemical researches to be made, as well as with a portion of the physical investigations, and he had ample opportunities of observing the method of conducting the deep-sea dredging. It may be stated broadly that three sets of enquiries were instituted—(1) an investigation into the temperature of the sea at great depths, with a view to ascertain the extent and direction of submarine currents supposed to exist; (2) an enquiry into the existence and distribution of animal life at these depths; (3) an examination of the sea-water at various depths for its physical and chemical properties, such as its specific gravity, the amount and nature of the gases dissolved in it, and the organic matter contained in it.

It will be convenient to describe—first, the separate parts of apparatus with which the vessel was furnished for the prosecution of these enquiries, and, subsequently, the way in which the whole was used.

H.M.S. *Porcupine*, paddle steamer, 380 tons, is one of the surveying vessels of her Majesty's navy, and has long been com-

manded by Staff-Commander E. K. Calver. To the extensive and varied experience gained by him in surveying, is to be attributed in great measure the unexpected success which he attained in this new work, many of the expedients which his ingenuity suggested being of the utmost value.

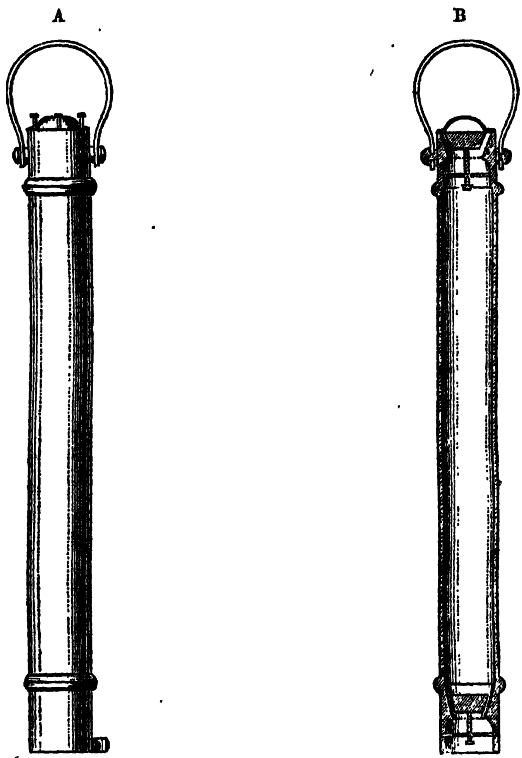
The sounding apparatus employed in deep water was the form adopted in H.M.S. *Hydra*, Captain Shortland, in sounding across the Indian Ocean, preparatory to the laying of a submarine cable. In order to obtain the depth correctly, it is necessary that the weight employed should be sufficiently heavy to make the sounding line run out with great rapidity. If this were not the case, and the line were payed out slowly, it would be liable to be deflected by currents, and by the drift of the vessel at the surface, especially in a breeze, during the progress of the operation, so that a greater depth would be indicated on the line than really existed. It is obvious, therefore, that in proportion as the depth is greater, and more line is employed, a heavier weight is required to counteract the increased friction of the water upon the line. In order to avoid the risk of breaking the line while hauling it in, and consequent loss of line and instruments, the following contrivance is employed, by which the weight becomes detached on striking the bottom. It consists of a cylindrical rod, about one inch in diameter, with a ring at the top to which the line is attached. A ring of iron slides loosely on the rod, and above this are cast-iron cylindrical weights, 1 cwt. each, perforated so that the rod passes freely through them. The ring and weights are kept in their place by a wire passing from the bottom ring to a small spring-catch above. As long as there is any strain on this apparatus, i. e. as long as it is descending vertically through the water, the weights are kept in their places, but when it strikes the bottom, the wire becomes slack, and is thrown off the spring, releasing the weights, so that when the line is hauled in again, the cylindrical rod withdrawn from the weights is the only part of the apparatus that returns, bringing with it a small portion of the sea bottom, retained in its lower part by an arrangement of valves which it is unnecessary to describe. The sounding line employed with this apparatus was specially made for this expedition, and though it was only 0·8 inch in circumference, its breaking strain was 12 cwt. It was marked at intervals of 50 fathoms, and kept for use wound on a large drum.

The apparatus employed for temperature determinations was the result of much forethought and of many experiments made by the Physical Committee of the Royal Society. The experience of former temperature-soundings tended to show that ordinary self-registering thermometers were liable, among other

errors, to indicate too high a temperature, from the pressure of the sea-water at great depths (which amounts to one ton per square inch for every 800 fathoms) slightly compressing the bulb and forcing the mercury, or spirit, too high up in the column. Many expedients were tried to protect the thermometers from this source of error, and ultimately the plan suggested by Professor W. A. Miller, Treasurer R. S., was adopted. A figure of the instrument, and a detailed description of it, will be found in the *Proceedings of the Royal Society*, June 17, 1869. It consists of the ordinary form of Six's self-registering thermometer, the bulb of which is surrounded by an outer bulb, hermetically sealed, and the intervening space partly filled with spirits of wine. A careful series of experiments made on shore with the assistance of a hydraulic press, showed that the pressure only acted upon the outer bulb, the inner indicating the true temperature, and that the difference in indications between this and an ordinary thermometer increased regularly with the pressure. These differences were carefully noted at intervals up to three tons to the square inch, and it was exceedingly interesting to find that a set of independent experiments made at sea, when the protected and ordinary thermometers were simultaneously employed, gave precisely the same differences between the two instruments when the pressure was calculated at the rate of one ton per square inch for every 800 fathoms of depth. In addition to the precautions against pressure, these instruments, to the construction of which Mr. Casella paid the utmost attention, were furnished with registering indices of a peculiar kind, which fitted so tightly in the tubes that they could not be displaced by the shaking to which they were occasionally subjected, notwithstanding the most careful handling, and which could only be set by powerful magnets, with grooves cut in the end of their poles, so as to partially surround the thermometer tube. The whole instrument was protected by a copper cylinder open at each end. The expedition was provided with six of these instruments, but the two which were originally employed were found to work so entirely satisfactorily that they were used for every observation, and it was calculated that they travelled vertically up and down in the sea more than a hundred miles during the three months that they were in use.

The apparatus for collecting samples of sea-water from various depths was of very simple construction. It consisted of a cylindrical brass tube (the interior being coated with varnish, to avoid the action of the sea-water upon it), provided at each end with an accurately ground valve opening upwards. As long as this was descending vertically, the water passed freely through it, but when the motion was reversed the

pressure of the superincumbent water kept the valves closed, and the water which last passed into the bottle was retained there. That "bottom water" really was brought up, the



Water-bottle as seen at A externally, and at B in section; drawn to a scale of *one-eighth* the actual size.

writer had ample evidence, as from the greatest depths it was cloudy with suspended mud, minute Globigerinæ, and other Foramenifera.

The dredges with which the expedition was provided were the ordinary form of "naturalist's dredge" introduced by Ball and Forbes. They were made of wrought iron, the scrapers being pitched at a very low angle, and the arms being moveable. One of the arms was in two pieces, terminating in rings, which were fastened together by a few turns of spun yarn, so that, when the dredge got fouled by rocks, as occasionally happened in depths less than 500 fathoms, the strain upon the dredge broke the "stop," the dredge changed its position, and freed itself. The loss of several dredges was avoided by this simple precaution. For very deep dredging, the weight of

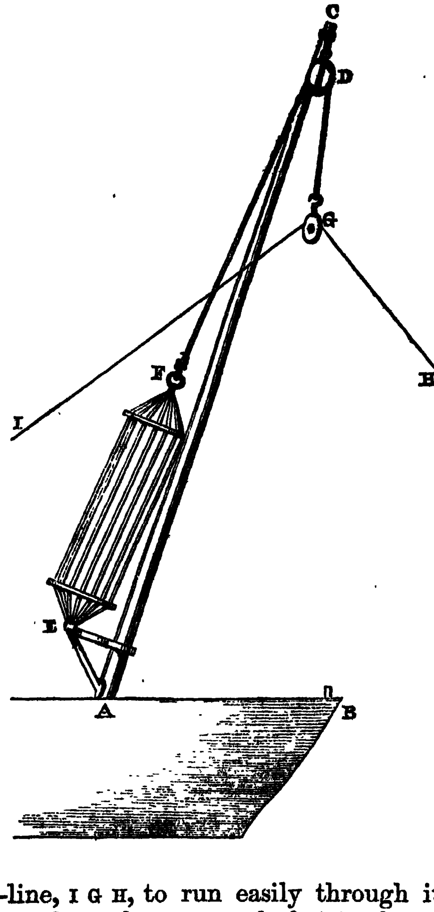
the frame was increased to about 2cwt. by the addition of plates of iron. The bag was double, the outer being a close net of sounding-line, the inner a piece of "bread bag," a somewhat open canvas, and was so arranged as to be readily detached from the frame, and washed, to avoid the possibility of mixing the results of one dredging with another. The animals &c. brought up in the dredge were readily separated from the mass of ooze or mud in which they were embedded by a series of graduated sieves, placed one within another, the sieve of largest diameter and smallest mesh being at the bottom of the series. When the whole was agitated in a tub of water, the impalpable mud speedily passed through all the sieves, and the animals were deposited upon them according to their size.

The dredge-rope was the best Chatham "hawser-laid" rope that could be made; two sizes were employed—2 inches and $2\frac{1}{2}$ inches in circumference respectively. There was an admirable arrangement for stowing it on deck, by which its manipulation was rendered very easy, which is represented in the full-page plate. A number of pins, about 18 inches long, terminating in white balls, projected inwards from the bulwarks, and on these pins the rope was loosely coiled. In this way, 3,000 fathoms of rope (nearly $3\frac{1}{2}$ statute miles), weighing nearly $2\frac{1}{2}$ tons, were placed within easy reach on the deck.

In order to haul in the great lengths of sounding-line and dredge-rope, as well as the dredge with its contents, a small donkey-engine was provided and placed on deck between the paddle-boxes. The experience of the 1868 expedition, in H.M.S. *Lightning*, had shown that a double-cylinder engine was almost essential, so that by the alternate action of the two pistons the application of the power might be uninterrupted. The engine on board the *Porcupine* fully answered the expectations formed of it, and in almost every case, whether it was sounding-line from a moderate depth, or the dredge and its contents with 3,000 fathoms of rope attached, the hauling-in was performed at the rate of one foot per second, or 600 fathoms per hour. Occasionally, in fine-weather soundings, a drum of greater speed was attached to the engine.

A very important adjunct to the dredging and sounding apparatus remains to be described. It is figured in the following cut, and its position on board the vessel is clearly seen in the full-page plate. It was called the "accumulator," and the twofold object of the contrivance was—to indicate any sudden and undue strain upon the dredge-rope or sounding-line, and to relieve it in a measure—automatically, as it were. The dredging was carried on at both ends of the vessel, a similar contrivance to that seen in the stern being fixed near the bow.

To one end of a stout spar, A C, was firmly lashed a block, D, and to one end of the rope which passed through this block was attached a smaller block, G, which allowed the dredge-rope



or sounding-line, I G H, to run easily through it. The other end of the rope through D was made fast to the "accumulator," E F, which consisted of a number of solid vulcanised india-rubber springs, about two feet long, connected at each end to a disc of wood. In the event of the dredge fouling among rocks, or the vessel pitching much in a sea, a sudden strain was put upon the dredge-rope and on the block G, and thence communicated by the rope G D F to the accumulator, E F, which immediately was stretched out, sometimes to three times its original length. On the *only* occasion when a dredge was lost, the writer saw a strain upon the indicator amounting

to 17 cwt. a few minutes before the dredge-rope parted. This happened in comparatively shallow water, on a day when what a landsman would call half a gale of wind was blowing.

The expedition was divided into three cruises; the first, under the charge of Mr. J. Gwyn Jeffreys, F.R.S., and accompanied by the writer, lasted from the end of May till the middle of July, and the area of its work lay between latitudes 51° 20′ N., and 57° 30′ N. (Rockall), the westernmost point being 15° 30′ W. The second, under the charge of Dr. Wyville Thomson, F.R.S., accompanied by Mr. John Hunter, M.A., of Belfast, lasted from the end of July to the middle of August, and was occupied in explorations off the S.W. of Ireland; the deepest dredging of all, 2,435 fathoms, being accomplished at the most southerly point reached, 47° 35′ N. and 12° 15′ E. The third, under the charge of Dr. Carpenter, F.R.S., accompanied by Dr. Wyville Thomson and Mr. P. H. Carpenter, lasted from the middle of August to early in September, and was devoted to an examination of the "warm and cold areas" between the Hebrides, Shetlands, and Faröes, the northern limit being Thorshaven, in the Faröes, the western 9° 20′ W., and the eastern 0° 35′ E. In this cruise the greatest depth attained was 750 fathoms, but during it a most important addition to the dredging apparatus was made by Capt. Calver. It consisted of horizontal arms attached to the dredge frame, to which were fastened two or three "swabs," or "hempen tangles" as they were christened by their inventor, by which the bottom of the sea was swept as well as scraped. In this way many animals were captured which seldom or never were obtained in the dredge-bag, and the utility of this contrivance was so evident that no deep-sea dredge can henceforth be considered complete without it.

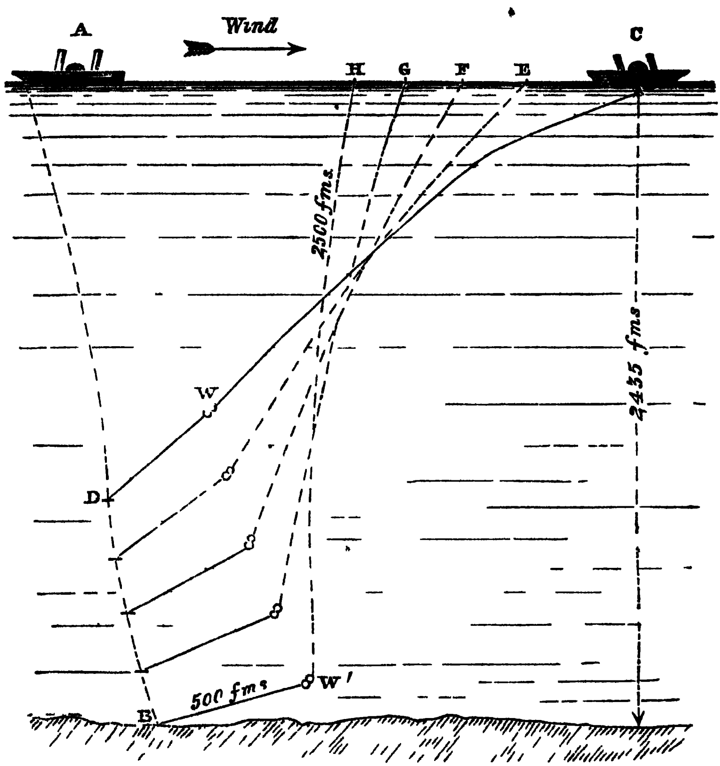
In order to dredge in any particular spot, it is necessary to know the exact depth of water. Sounding, therefore, is an essential preliminary to dredging. In the *Porcupine's* soundings three objects were attained—(1) a knowledge of the exact depth, (2) the temperature at that depth, (3) a sample of water from the bottom. Where there was reason to believe that the depth was less than 1,000 fathoms, an ordinary "deep-sea cup-lead," weighing 1 cwt., was used; in other cases the "Hydra" machine before described. The sinker or weight having been attached, two of the Miller-Casella thermometers were made fast to the line immediately above the weight, and close to them the water-bottle. The whole was then let go, the line running out with great rapidity, at the rate of 100 fathoms per minute at first. By a seconds' watch, the time of each 100 fathoms in running out was noticed, the interval increasing with the friction of the water on the greater length of

line, so that towards the end of a 1,200 or 1,300 fathoms' sounding between two and three minutes were required for 100 fathoms to run out. Capt. Calver took each sounding with his own hands, and was able to tell with great certainty the precise moment when the weight struck the ground. In the fine weather with which we were usually favoured, the sounding-line even at these great depths hung quite vertically over the stern of the vessel, and the bottom could be felt at the end of it, by slightly raising and lowering the line by hand. Five minutes were then allowed for the thermometers to take up the temperature of the surrounding water, after which the line was hove-in by the donkey-engine, and when the instruments arrived at the surface the indices of the thermometers were read and the results recorded. Soundings were taken in this manner at ninety different stations, and when it was desired to take the temperature of the sea, and samples of water at various vertical depths (short of the bottom) over the same spot, the operation was repeated with the cup-lead, letting it down, e.g., to 1,250, 1,000, 750, 500, and 250 fathoms, and drawing it up again after each operation. In this way the *serial* temperature soundings were obtained.

The contents of the water-bottle were immediately examined for their specific gravity by delicate glass hydrometers reading to four decimal places; for the organic matter contained in them by the chameleon (permanganate of potash) test; and for the quantity and relative proportions of gases dissolved in it. This last point was determined by Dr. W. A. Miller's apparatus, which was first adapted to the necessities of ship-board by the writer. The gases were expelled from the water by boiling it in such a way that no air had access to it, and were collected over mercury and measured. The carbonic acid was then absorbed by potash, and the oxygen by pyrogallic acid; the remainder being taken as nitrogen. In addition to being thus tested, some of the contents of the water-bottle were preserved for further analysis on shore.

It now remains to describe the process of deep-sea dredging proper. In water less than 500 fathoms' depth, two dredges were frequently employed, one from the bow, another from the stern; but at greater depths only one dredge was used. The object to be attained was to get the dredge to fall vertically through the water to its right position on the bottom, over which it was to be dragged by the motion of the vessel at the surface. In small depths this was a matter of little difficulty, as the time occupied by the dredge in falling to the bottom was so short that the vessel did not drift much from one spot, and then, if the drift of the vessel was not enough, recourse was had to the paddles to move the dredge over the ground.

Where the depth was great, however, and especially when the surface drift was considerable, special precautions had to be observed, the chief of which was to move the vessel by steam up to windward, so as to counteract the leeway. An additional device was to attach a weight (1 or 2 cwt.) to the dredge rope at some distance from the dredge—usually about one-fourth of the known depth. The annexed figure (for which, as for the other intercalated cuts, the editor is indebted to the Secretary of the Royal Society) gives an idea of the relative positions of the vessel and the dredge according to this plan of dredging.



A represents the position of the vessel when the dredge is let go; A B the line of descent of the dredge. While the dredge is going down, the vessel drifts to leeward, say to C, when C W D would represent the relative positions of the vessel, the weight, and the dredge itself. E, F, G, H, are the positions of the vessel in steaming to windward, during which time the dredge sinks from D to B, and the weight from W to W'. This operation is repeated two or three times, and the vessel is then

allowed to drift, and to carry forward the weight w^1, so that the dredging is carried on from the weight, and not directly from the vessel. In this way the dredge is quietly pulled along, its lip scraping the bottom in the attitude which it assumes from the position of the centre of gravity of its iron frame and arms, and a much smaller quantity of rope is required in proportion to the depth than in ordinary in-shore dredging. For the 2,435 fathoms' dredging, 3,000 fathoms of rope were employed, and during the time when the writer was on board, 2,000 fathoms of rope were used for depths ranging from 1,200 to 1,476 fathoms.

It is with great satisfaction that we have heard that her Majesty's Government have granted the use of the *Porcupine* and her apparatus for a cruise in the Bay of Biscay and the Mediterranean during this summer, under the same scientific charge as before, and that her talented and experienced captain will be still in command of her. The scientific public will await the results of this expedition with great interest.

EXPLANATION OF PLATE.

The plate represents the quarter-deck of the *Porcupine* in Galway Dock, June 1869 (from a photograph).

REVIEWS.

OTHER WORLDS THAN OURS.*

IT seems as if each department of science had its own especial epoch or period, when, by reason of its popularity, or the importance of the discoveries revealed by those who belong to it, it becomes more prominent than usual. Just in such an epoch does astronomy appear to be at the present time. The great improvements in telescopic construction, the comparative cheapness of instruments with which original research can be conducted, the wonderful applications of spectroscopy and photography to astronomical enquiries, all have helped to render the study of the heavens, not only a pursuit open to thousands who hitherto never looked at a star, but have also by the revelation of some of the most wonderful phenomena in nature, given astronomy an importance which it hardly had before.

The time, therefore, had come when a treatise embodying the results of later researches, and giving, as it were, a bird's-eye view of our knowledge of the celestial bodies, as it now exists, was required; and such a treatise Messrs. Longmans have given us in the excellent volume now under notice. Mr. Proctor—who is at once a teacher, an investigator, and a writer—combines the three qualities essential to the construction of such a work as that to which we have alluded, and he has brought these into full and successful operation in his account of the modern heavens, if we may use the expression. His book is not a treatise on astronomy; it is more a work which, in the fashion of Herschel's *Lectures on Scientific Subjects*, deals fully, and yet with remarkable clearness, with a number of scientific problems, and which is addressed to the man of general education, whether he be an astronomer or not. But as the author treats of nearly all the astronomical questions which come within the range of the ordinary text-book, the person who carefully reads *Other Worlds than Ours* will learn nearly all that the scientific star-gazer has got to tell.

When we tell our readers that Mr. Proctor has chapters on the Earth, the Sun, the inferior planets, on Mars, Jupiter, Saturn, Uranus, the Moon, meteors, and comets, the stars, and the nebulæ, they will excuse our at-

* "Other Worlds than Ours: the Plurality of Worlds, studied under the light of recent scientific researches." By Richard A. Proctor, B.A., F.R.A.S. London: Longmans, 1870.

tempting a general analysis of the work. We may, however, dwell on some of its more novel and interesting features. These, it appears to us, are to be found in the chapter entitled, "What we learn from the Sun." In this section of his work, which is essentially controversial, the author enters upon a discussion of the several moot points in connection with the late solar observations. In reference to the sun's corona and the zodiacal light, subjects which readers will be aware have of late received considerable attention, Mr. Proctor has much to say. He enters with considerable minuteness into the consideration of Mr. Lockyer's hypothesis on the origin of the sun's corona, and he adduces certain very formidable arguments against these views and in favour of his own opinions. It would be impossible to put the author's observations into a smaller compass than he has himself compressed them, and as they occupy several pages we could not possibly reproduce them in full. We, however, may just quote one passage, which, to a certain extent, contains the pith of Mr. Proctor's ideas on the point in question. After commenting on the spectroscopic evidence as to the corona and on the "atmospheric glare" theory of Mr. Lockyer, he goes on to say:—

"Now, remembering that we have two established facts for our guidance —(1) the fact that the corona cannot be a solar atmosphere, and (2) the fact that it must be a solar appendage—I think a way may be found towards a satisfactory explanation. Let it be premised that the bright lines of the coronal spectrum correspond in position to those seen in the spectrum of the Aurora, and that the same lines are seen in the spectrum of the zodiacal light, and in that of the phosphorescent light occasionally seen over the heavens at night. Since we have every reason to believe that the light of the aurora is due to electrical discharges taking place in the upper regions of the air, we are invited to the belief that the coronal light may be due to similar discharges taking place between the particles (of whatever nature) constituting the corona. Now, though the appearance of an aurora is due to some special terrestrial action (however excited), yet the material substances between which the discharges take place must be assumed to be at all times present in the upper regions of air. In all probability they are the particles of those meteors which the earth is continually encountering. And since we know that meteor-systems must be aggregated in far greater numbers near the sun than near the earth, we may regard the coronal light as due to electrical discharges excited by the sun's action, and taking place between the members of such systems. Besides this light, however, there must necessarily be a large proportion of light reflected from these meteoric bodies. In this way the peculiar character of the coronal spectrum may be readily accounted for. We know from the auroral spectrum that the principal bright lines due to the electrical discharges would be precisely where we see bright lines in the coronal spectrum. But besides these there would be fainter bright lines corresponding to the various elements which exist in the meteoric masses. These elements we know are the same as those in the substance of the sun. Thus the bright lines would correspond in position with the dark lines of the solar spectrum. Hence, as light reflected by the meteors would give the ordinary solar spectrum, there would result from the combination a continuous spectrum on which the bright lines first mentioned would be seen, as during the American eclipse."

Such is Mr. Proctor's ingenious theory. In the present condition of astronomical science it is impossible to pronounce definitely in its favour; but it certainly fascinates us by its simplicity, and by the fact that it collates a number of otherwise heterogeneous facts; whilst it is certainly less of the haphazard and "dernier ressort" character than Mr. Lockyer's "glare" notion. At all events, whatever be its merits, or that of any of the other original ideas with which the book abounds, it is clear that Mr. Proctor's volume is a most attractive and instructive one, and one which should be carefully read by all intelligent persons who come under Mr. (now Dr.) Matthew Arnold's category.

PTERODACTYLES.*

IT is not many years ago that the remarkable fossils of the Cambridge Greensand which Mr. Seeley has now so well described were a veritable *terra incognita* to the palæontologist. Till the publication of the present excellent volume, it would have been difficult for the worker to obtain in any one treatise all the information he might have desired as to the affinities and the remains of the Cambridge Pterodactyles. But now, thanks to the author's labours, we have in this treatise an elaborate account of all that relates to the palæontological history of the Cambridge Greensand Ornithosauria. Mr. Seeley has gone elaborately into his subject, so that indeed his book may well be regarded as a handy monograph on pterodactyles generally. In his earlier chapters he gives a sketch of the organisation of these curious reptiles, and an account of the fossil remains on which the earlier descriptions of the group were founded. Next he states in outline the views taken of these affinities by Cuvier, Sömmerring, Oken, Wagler, Goldfuss, Wagner, Quenstedt, Burmeister, and Von Meyer. Then, without basing his own opinion on those which have been enunciated by previous writers, he proceeds to reason on the *à priori* method, and adopts the view that the pterodactyles were neither birds nor reptiles, but were a group between the two, and entitled to a place of equal rank with the class birds. The following epitome, nearly in the author's words, gives a notion of his doctrine:—

"The Pterodactyles have a nervous system of the bird type; they have a kind of brain which exists only in association with a four-celled heart and hot blood; they have a respiratory organisation which only exists among birds: with that respiratory apparatus is always associated a four-celled heart and hot blood, which it would necessarily produce; and with that respiratory organisation is always associated a brain of the type that the Pterodactyle is found to possess. Therefore," says Mr. Seeley, "*it is firmly indicated that the general plan of the most vital and important of the soft structures was similar to that of living birds.*" He then goes on to say that these avian

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* "Ornithosauria: an elementary study of the bones of Pterodactyles made from fossil remains found in the Cambridge Upper Greensand." By Harry Govier Seeley, of St. John's College, Cambridge. London: Bell & Daldy, 1870.

characters are, however, associated with such a "diversity of details" as to demonstrate that Pterodactyles must be placed in a new group, of equal value with birds, and called Ornithosauria. It is impossible to be perfectly convinced that Mr. Seeley is right in the conclusion he so firmly lays down, for, after all, it is based very much on analogy, and on the supposition that the vague laws of correlation are without exception. But as we know that the law of correlation is occasionally a deceptive guide, it is possible that it may be uncertain even in Mr. Seeley's case, and that, after all, the Pterodactyle may have been a true reptile. If it is to be regarded as an avian-reptile, then the conformation of its organs of flight is in itself so wide a departure from the law of correlation as to throw considerable doubts on Mr. Seeley's notion. Still it must be confessed that the author has brought to bear on his theory a mass of evidence of the weightiest kind, and that he has studied the group very deeply, and under the immense advantages which the Woodwardian Museum offers; and hence we should be cautious even in exhibiting our scepticism. Whatever view may be taken of Mr. Seeley's doctrine, his book must be regarded as an admirable work of reference, ample in its details, and well arranged as to its bibliographical information. Its plates are numerous, but are, we must say, rather badly drawn and "smudgily" printed.

THE FUEL OF THE SUN.*

THERE is nothing, politicians tell us, like a healthy opposition, and we think that this is as true of the world of science as it is of politics. The author of the present work does not sit on the "Treasury Benches" in Science. He is nothing if not heterodox; but it gives us pleasure to say that his heterodoxy is of the best kind, it is calm, learned, and philosophic. If Mr. W. M. Williams differs in his opinion of the universe from the leading astronomers of the present day, he does so so modestly, with such good taste, and with such a proper display of the knowledge of his subject, that we are bound to listen to him respectfully and with attention. His book is not a small one, and it deals with a multiplicity of complex questions concerning the constitution of the sun and the existence of an universal atmosphere, and it would be out of our power to attempt here to grapple with views which we believe to be unsound and based on an insufficient practical study of the subject. Strange as it may seem, knowledge of natural phenomena, such knowledge as will enable the student to form correct opinions, cannot be obtained from books. There are little facts, small points in observation, which always incline the scale to this side or that, and which cannot be put down in words; nothing but practical work and research can enable the student to realise and appreciate them, and it is these which not seldom help him to conclusions. This knowledge it does not appear to us that Mr. Williams possesses, and for this reason we think he would have done better had he not published the present treatise. His ideas on the

* "The Fuel of the Sun." By W. Matthieu Williams, F.C.S. London: Simpkin & Marshall, 1870.

subject of an atmosphere through space, and on the question of electric induction, are most ingenious, but we fear that is all that can be said in their favour. The book itself deserves to be read, it is full of instructive matter and is well written.

PHRENOLOGY.*

WE have no objection whatever to the idea of Phrenology in the abstract. The tendency to localization of function may probably hold good in the brain as it does elsewhere in the animal kingdom, but as we do not know yet of how many separate parts mental phenomena consist, and as we are certainly in the most profound ignorance as to what portions of the brain are directly associated with any individual mental operation, we must scout the idea of the "popular phrenology" of Dr. Donovan's school as the merest, the wildest chimera, and we must regret its existence because of the stupid nonsense which it helps to spread. But of all the essays on Phrenology which we have seen, this one of Dr. Donovan's has the least claim to be regarded as scientific. It is really the most utter rubbish, so far as its scientific value is concerned, and the fact that its appendix contains letters testifying to the author's skill in detecting the aberrations of the upper table of the skull (which by no means correspond to the contour of the brain) shows that the work is a mere advertisement by a person who practises phrenology as a profession.

CURIOSITIES OF TOIL.†

SOME time ago a writer to the *Daily News* made the acute suggestion that a man who has a faculty for the device of "taking titles" should be permitted to possess a copyright entitling him to a reward for his ingenuity. The idea is not at all so extravagant as may at first appear. Publishers know well the value of a "catching name" for a work or periodical, and we know of one eminent dealer in books in London, who having made a journal a success simply through its title, left the originator of that title no remuneration whatever beyond the verbal recognition of his tact. We can only hope that some day the *Daily News'* scheme will be carried out and that it will be made wholesomely retrospective. If it should be, we are certain that a small fortune will accrue to the author of the highly interesting volumes before us, for if there be a writer among us who has a happy knack of hitting upon pithy, successful, attractive titles for his books, that writer is Dr. Andrew Wynter. The two volumes which Messrs. Chapman & Hall have issued are the reprint of several articles which the author

* "A Handbook of Phrenology." By C. Donovan, Professed Phrenologist. London: Longmans, 1870.

† "Curiosities of Toil" and other Papers. By Dr. Wynter. 2 vols. Chapman & Hall, 1870.

from time to time lately contributed to the *Times*, the Quarterlies, and other journals. These articles deal with some very novel industrial resources, and though many of them cannot be regarded by the political economist as seriously worthy of study, the ordinarily educated person, the railway traveller, the man who desires to relieve his brain by half an hour's pleasant and yet instructive desultory reading, will be grateful to Dr. Wynter for his volumes. Among some of the more important and scientific contributions to the "Curiosities of Toil," may be mentioned the "Use of Refuse," "Tricks of the Wine Dealers," "Bodily Repairs," "Special Diseases of Artizans," "Homes without Hands," "Earth Closets," and Fish "Farming." By the way, in reference to Dr. Wynter's two papers on Fisheries and Fish Farming, we would point to a couple of errors in proper names, which we trust the author will correct in the next edition, for the benefit of those who may be desirous of pursuing the subject further. Dr. Kemmer should be Dr. Kemmerer, and M. Cost*a* should be M. Coste. In conclusion, let us point out (what is perhaps unnecessary in the case of a *littérateur* so known to fame), that Dr. Wynter's books are not only remarkable for the interesting facts they bring together, but for the brilliancy, terseness, and epigrammatic character of the author's style.

PRIMITIVE MAN.*

HERE is another of M. Figuier's handsome compilations, and on the whole not a bad one. The writer has gone out of the beaten track in search of a subject, and though his work is not, as he imagines it, the only general treatise on the subject, and though in point of accuracy it is far behind the treatises of Keller and Nilsson and Sir John Lubbock, it is nevertheless an interesting and useful volume. As its name indicates, it deals with the great questions which of late years have cropped up in connection with what is called prehistoric archæology. It treats of all the evidence, geological and palæontological, by which the antiquity of man is established; and if the compiler would have contented himself with stating the opinions those who have conducted researches on the subject, and have resisted the temptation to offer his own valueless ideas, the book would have been a more profitable one than it is. The cuts which are to be found on every page are well done, and form the finest collection of illustrations of prehistoric human relics that is to be found anywhere; but the ideal and "artistic" plates are simply execrable productions. The editor of the English edition has, we observe, very properly dissented from some of M. Figuier's rather startling perversions of fact, and has, so far as we can see, excised some of the French writer's useless, impertinent, and shallow critiques of the labours of our best English geologists. The book is one which we can recommend, and our thanks are due to Messrs. Chapman & Hall for introducing it to the British public.

* "Primitive Man." By Louis Figuier. Revised translation. London: Chapman & Hall, 1870.

NATURAL SELECTION.*

MR. WALLACE, to whom science is nearly as much indebted for the theory of Natural Selection as it is to Mr. Darwin, has done wisely and well to reprint, in the convenient form of a volume, the more important of those scientific papers in which from time to time he advocated and supported the now generally accepted doctrine of the origin of species. It will be unnecessary for us to enter critically into Mr. Wallace's chapters, as the several articles have already been for years before the public, and have been dealt with by both scientific and general reviewers. We would, however, refer to one of them as of surpassing interest, for in it the author attempts an intensely difficult problem in trying to apply the general evolutional doctrine to the phenomena which are popularly included under the term instinct. The remarks in this chapter have not hitherto been published. The author's definition of instinct seems to us to be especially a good one, and to be sufficiently comprehensive to include all the operations which properly come within the term. Mr. Wallace says that instinct is "the performance by an animal of complex acts absolutely without instruction or previously acquired knowledge." This is perfectly satisfactory, and though it does not seem to us that in the present state of science Mr. Wallace is in a position to explain the more curious examples of instinctive acts, he has certainly gone far towards convincing us that instinct is not to be explained by any reference to a supernatural law. His remarks on the questions, "Does man possess instinct?" and "How Indians travel through unknown and trackless forests," are admirable examples of candour in acknowledging difficulties, and of clear reasoning on the facts as we know them. Mr. Wallace's book is one which is intelligible to an ordinarily clear-headed person, and it is of course a standard book which every biologist must have on his bookshelves. The following is a list of the chapters it contains, with the dates of their first publication:—"On the Law which has regulated the Introduction of New Species" (September 1855); "On the Tendency of Varieties to depart indefinitely from the Original Type" (August 1858); "Mimicry and other Imitative Resemblances among Animals" (July 1867); "The Malayan Papilionidæ, or Swallow-Tailed Butterflies, as Illustrations of the Theory of Natural Selection" (March 1864); "On Instinct in Man and Animals" (not before-published); "The Philosophy of Birds' Nests" (July 1867); "A Theory of Birds' Nests" (1868); "Creation by Law" (October 1867); "The Development of Human Races under the Law of Natural Selection" (May 1864); and, lastly, "The Limits of Natural Selection as applied to Man."

THE INTERIOR OF THE EARTH.†

THE reader of this work will do well, in the first instance, to understand clearly that the author is not the eminent authority on earthquakes and volcanic action, but is an outsider in science, who, from a few imperfect

* "Contributions to the Theory of Natural Selection: a Series of Essays." By Alfred Russel Wallace. London: Macmillan, 1870.

† "The Interior of the Earth." By H. P. Malet, E.I.C.S. London: Hodder & Stoughton, 1870.

observations of his own, and a limited knowledge of the literature of his subject, has ventured to discuss the question as to whether the interior of the earth is a mass of liquid fire. The book is not badly written, it is remarkably well printed, and its mechanical features are generally good. But in other respects it is a volume which, though it may gratify the *amour-propre* of the author, can serve no useful purpose either to science or theology. Mr. Malet seeks to show that the interior of the earth is not fire, and that the present shape of the globe is due chiefly to the influence of water in some form or other. His text is, "The Spirit of God moved upon the face of the Waters," and, starting with this, he displays much of the ingenuity of a special pleader in urging his cause. As a whole, however, the work deserves no further notice at our hands.

A MANUAL OF ZOOLOGY.*

HAS not Dr. Nicholson infringed Mr. Renshaw's copyright in giving his book the same title as that of the English edition of M. Milne Edwards' well-known handbook? The question is one of importance, though it does not affect the reviewer. Of the author's labours in producing this work we cannot speak in very favourable terms. The book is too diffuse in some parts, and too concise in others. Yet is it a work which in point of accuracy far surpasses most of the Manuals which of late years have issued from the press. It is devoted exclusively to the subject of the Invertebrates, and, so far as we can see, it consists very nearly entirely of a paraphrasis of Huxley's recent volume, and the two admirable little manuals of Professor J. Reay Greene. This, it seems to us, is hardly fair. When Mr. Greene wrote his Manuals, he had all the labour of going to the actual fountain-heads for information, and as a consequence he produced a work which for exactitude and condensation stands *facile princeps.* But practically, this Manual of Dr. Nicholson's is the three volumes we have referred to rolled—like the proverbial single gentleman—into one. The illustrations are not at all numerous enough, and they are barbarously printed. We believe that, though Mr. Hardwicke's name is on the title-page, he has ceased to be the publisher of the work.

GRAVE-MOUNDS AND THEIR CONTENTS.†

ONE would hardly have thought that so limited a branch of archæology as that of grave-mounds could have been made the subject of a volume of three hundred pages, and yet the book before us deals almost exclusively with the history of these objects, and is by no means a made-up work, but

* "A Manual of Zoology, for the Use of Students, with a general Introduction on the Principles of Zoology." By Henry Alleyne Nicholson, M.D., D.Sc. Vol. i., Invertebrate Animals. London: Hardwicke, 1870.

† "Grave-Mounds and their Contents: a Manual of Archæology as exemplified in the Burials of the Celtic, Romano-British, and Anglo-Saxon Periods." By Llewellyn Jewitt, F.S.A. London: Groombridge, 1870.

a genuine and interesting record of observations made by one who not only understands but who loves the particular line of research he has undertaken. Mr. Jewitt has here given us an account, accompanied by nearly five hundred capital woodcuts, of the grave-mounds "of the three great divisions of our history—the Celtic, the Romano-British, and the Anglo-Saxon." The author's treatment of his subject has no relation to geological doctrine, and has not much to do with Early English history; it is essentially a matter of fact account based on very extensive original investigation. To the general reader, it seems to us that the author's observations on the Celtic mounds will prove the most interesting, as they are the least familiar, and the author's excellent sketches of the half-opened graves with the skeletons *in situ* give such an excellent idea of the methods practised, that, even without the text, they form in themselves a very succinct history. The other periods, having been less or more treated upon in other essays, are less new to the reader, though much that Mr. Jewitt has to say about them is novel. We commend this work highly; it is a book *sui generis*, and both the author and publisher have done their parts well.

THE LAWS OF MAGNETISM.*

PROFESSOR TYNDALL'S last great treatise has so recently come into our hands, and is so vast a work and deals with such a multitude of the most important and least known phenomena of magnetism, that we can do no more than give a very brief outline of the nature of its contents.

In the first place we may state that the book—which is a large octavo, including over 350 pages—is, in great measure, the reproduction of the papers which the author has contributed to purely scientific journals during the last eighteen years. It deals with the great question of the relation of magnetic force to the external world—animate and inanimate—and from these developes those great laws which the painstaking research of Faraday first revealed. Hence it has an interest, not only for the physicist, who must study it with the closest attention, but also for the general reader, and not less for the medical man, who, interested in electro-physiology, may gather from it something to indicate the origin of the curious effects which powerful magnets appear to have upon the human frame. It would be quite useless for us to attempt, even in the most popular form, an analysis of the author's opinions—the views of one who has devoted a lifetime of [illegible]ch to their formation—so we will close our notice with the titles of the several sections into which the treatise is divided:—Magneto-optic Properties of Crystals and Relation of Magnetism and Diamagnetism to Molecular Arrangement; Diamagnetism and Magne-Crystallic Action; Polarity of the Diamagnetic Force; On the Polarity of Bismuth, including an Examination of the Magnetic Field; The Nature of the Force by which Bodies are Repelled by the Pole of a Magnet; Further Researches on the

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* "Diamagnetism and Magne-Crystallic Action, including the question of Diamagnetic Polarity." By John Tyndall, LL.D., F.R.S. London: Longmans, 1870.

Polarity of the Diamagnetic Force; Faraday's Letter to Matteucci on Diamagnetic Polarity; The Relation of Diamagnetic Polarity to Magne-Crystallic Action; Faraday on Magnetic Polarity; A Magnetic Medium in Space; Thomson on the Magnetic Medium, and on the Effects of Compression; Weber on Diamagnetism; On Reciprocal Molecular Reduction; Thomson on Reciprocal Action of Diamagnetic Particles; Faraday on Magnetic Hypotheses; Mechanical Effects of Magnetisation; Influence of Material Aggregation on Manifestations of Force; Diamagnetic Repulsion; Disposition of Force in Diamagnetic and Paramagnetic bodies; Currents of the Leyden Battery; Influence of Magnetic Force on the Electric Discharge; The Magnetic Field and the Electric Current; Reduction of Temperatures by Electricity; The Polymagnet: Magnus' Investigation of the Electric Currents; Kohlrausch's Verification of the Theory of Ohm; and lastly, On Electro-Magnetic Attractions.

RUSTIC ORNAMENTATION.*

THOUGH not a scientific work, this is a book which, as it tells us much concerning the best and most artistic methods of constructing aquaria, fern-cases, and window-gardens, must find a place on the drawing-room table of all who are not only interested in scientific entertainment, but are also gifted with an eye for what is pretty and ornamental. It is a handsome volume, showily illustrated, richly bound, well printed, and containing a great many useful hints borrowed from the experience of a gentleman who is no mean authority on the subject he has taken in hand.

Alpine Flowers for English Gardens. By W. Robinson, F.L.S. London: John Murray, 1870.—Mr. Robinson has in this work put together a quantity of information which no doubt will be of great interest to those who are desirous of extending the limits of the ordinary garden flora. The illustrations are not numerous, but are to the point.

Notes of a Course of Nine Lectures on Light, delivered at the Royal Institution of Great Britain, April 8 to June 3, 1869. By John Tyndall, LL.D., F.R.S. London: Longmans, 1870.—These notes comprise an extended syllabus of the lectures delivered by Dr. Tyndall. They form valuable headings for those who desire to pursue a systematic and advanced course of optical studies, and for the junior student they constitute an excellent manual —we should say the best in the English language.

Irregularities of the Teeth. By Henry Sewill, M.R.C.S., &c. London: Churchill, 1870.—This is a reprint of some very good practical papers published originally in the *Lancet* and *British Medical Journal.* Mr. Sewill understands his subject, and displays great practical skill in the application of his art to the removal of dental deformities.

* "Rustic Adornments for Houses of Taste." By Shirley Hibberd. London: Groombridge, 1870.

Observations on Fundamental Principles and some Existing Defects in National Education. By Neil Arnott, M.D., F.R.S. London: Longmans. —Dr. Arnott here gives us a new edition of a little book which is not devoid of interest, especially at the present time, when educational questions are cropping up.

Science for the People. By Thomas Twining, V.P.S.A. London: Goodman, 1870.—The originator and proprietor of the Twickenham Economic Museum gives in this *brochure* some useful advice to those connected with the scientific education of the people. Mr. Twining is an authority on his subject, and his remarks deserve consideration.

A New Star Atlas for the Library, the School, the Observatory, showing all the stars visible to the naked eye, and fifteen hundred objects of interest, in twelve circular maps on the equidistant projection, and picturing the heavens for the first time on a systematic plan, without appreciable distortion and within a convenient volume; with two index plates, in which the six northern and the six southern maps are exhibited in their proper relative positions, all the stars to the fifth magnitude being shown; with coloured constellation figures. Drawn by R. A. Proctor, B.A., F.R.A.S., and photolithographed by A. Brothers, F.R.A.S., with a letter-press introduction. London: Longmans, 1870. Almost as we go to press, Mr. Proctor's magnificent folio atlas reaches us. We have neither time nor space to do justice to its great merits, but we desire at least in this number to call attention to the fact that it is published, and that our best anticipations as to its good qualities are realised. The title is a little long, but happily, in this instance, it serves to state the general character of the work.

SCIENTIFIC SUMMARY.

ASTRONOMY.

Browning's Automatic Spectroscope.—The most important astronomical event during the past quarter is one which bears altogether on the future of astronomy. We are convinced that when ten years hence astronomers look back on the discoveries made during the decade then closing, they will find the most important of those discoveries associated in a more or less intimate manner with the work done by Mr. Browning's new spectroscope. In the hands of a Huggins or a Lockyer (and we doubt not that the instrument will soon find its way into all our leading observatories), a spectroscope which enables the observer to study each part of the spectrum as perfectly as though the instrument were specially constructed for the examination of that particular part alone, cannot but effect most important discoveries. It has been remarked that Mr. Browning will not find it easy to surpass himself after this invention: but we are no believers in scientific finality; so that, besides rejoicing at the promise given by the new instrument, we look upon it as affording yet more encouraging hopes of what Mr. Browning will effect hereafter in the way of instrumental improvements. We are glad to see, too, that he can appreciate the inventions of others, and has already begun to supply observers with Mr. Airy's ingeniously-contrived eyepiece for diminishing the effects of atmospheric chromatic dispersion. In observing Jupiter, when that planet returns to our skies next autumn, this eyepiece will be specially useful, since the planet will be low, and the colours—to which Mr. Browning first directed the attention of astronomers last year—will not be well seen through the denser atmospheric strata unless Airy's eyepiece be employed.

The Eclipse of December next.—Very promising accounts reach us respecting the expeditions which are to leave our shores next December to view the total solar eclipse in Spain and Sicily. Messrs. Huggins and Lockyer respectively head the spectroscopical corps; Messrs. Browning and Brothers the photographers; Professor Pritchard heads the polariscopic observers belonging to the Spanish expedition, but as yet no head has been appointed to the corresponding corps of the Sicilian party. It is not likely that under leaders so skilful anything but unfavourable weather can cause the failure of either expedition; so that astronomers may confidently hope

that discoveries of importance will be made before the end of the year, respecting the moot question of the solar corona.

The Sun's Corona.—In a paper communicated to the Royal Astronomical Society (see notices for March) Mr. Proctor treats of the sun's corona. He remarks that "The total eclipse of next December will last so short a time, that, if possible, no part of that time should be wasted through a misapprehension of the nature of the phenomena to be dealt with; so that it would be a matter to be much regretted that mistaken views should be promulgated, supposing it possible to form just ones." He then considers the evidence we have against the theory lately put forward that the corona is a terrestrial phenomenon. In the first place, he points out that the moon is projected as a dark disc against the bright background of the corona. The theory that the corona is a terrestrial phenomenon requires that the corona should be a foreground, and not a background, to the moon; and one may reasonably inquire how the moon, which lies beyond the earth's atmosphere, should come to be apparently projected upon the supposed glare of that atmosphere. But when the actual position of the moon's shadow, considered with reference to tridimensional space, is taken into account, it appears that during the period of total obscuration the atmosphere in the direction of the corona is not illuminated at all by the sun's rays. The whole of the atmosphere above the observer's horizon-plane is in partial shadow; that part from which, according to Mr. Lockyer's theory, the corona's light comes, is in total shadow; and if there is one atmospheric region of this part in total shadow which should appear darker than the rest, it is precisely that which corresponds in direction with the brightest part of the corona. Mr. Proctor then turns to subsidiary evidence. He remarks that atmospheric glare must undoubtedly exist before totality, and that if his views about the absence of glare after totality are correct (that is, if it is reasonable to inquire where the glare is to come from), then we might expect certain peculiarities to characterise the real glare which exists before totality. One such peculiarity is this, that the glare should cover the moon's disc until a moment or two before totality, and be visible in the act of passing off that disc. Now, in Col. Tennant's first photograph of the eclipse of August 1868, this glare is actually seen, the instantaneous slide having been removed (owing to a misunderstood order) a moment before totality had commenced. Again, during an annular eclipse the glare ought to be seen trenching on the moon's disc and leaving its central parts darker; and accordingly we find that such a phenomenon has more than once been recognised. Yet again, the moon's face ought to be visible by reflected earth-light, and more than once the features of the lunar surface have been thus seen during a total solar eclipse. Again, if the corona be a solar appendage, it should be visible a few seconds before and after totality: in 1860 Fr. Secchi saw the corona for 40 seconds after totality, and, in the total eclipse of 1733, M. Edstrom observed the radiations of the corona maintain an unchanged position as they gradually faded out of view with the increasing solar light. Mr. Proctor proceeds to adduce evidence in favour of the theory that the corona is but the richer portion of some appendage which causes the phenomenon of the zodiacal light.

Weather to be Expected during the Approaching Eclipse.—Lieut. Brown

has submitted to the Royal Astronomical Society some extracts from meteorological notes taken in Gibraltar from 1860 to 1867, and further information respecting the weather since the last date. The extracts, &c., refer to the last fortnight in December, and the following summary cannot but be regarded as affording the Spanish eclipse-expedition very good promise.

There were from December 15 to December 31 in the year

1860	6	very good days,	6	fair days, and	5	indifferent
1861	3	"	5	"	9	"
1862	10	"	6	"	1	"
1863	13	"	3	"	1	"
1864	5	"	10	"	2	"
1865	12	"	4½	"	½	"
1866	6	"	9	"	2	"
1867	8	"	5	"	3	"
1868	11	"	5	"	1	"
1869	7	"	9	"	1	"

Floor of Plato.—Mr. Birt continues to collect observations having reference to the degree of visibility of spots upon the Floor of Plato. The number of spots seen on the Floor of Plato up to the present time is 35, eight of which have been detected since Sept. 27, 1869. Since then no less than 531 observations have been made. The degree of visibility of each spot is indicated in a long and elaborate table, the indications of which are thus summed up by Mr. Birt:—"The number of spots, in which an increase of visibility has taken place, is nearly equal to that in which a decrease has occurred, viz., ten of the former and eleven of the latter. Spot No. 3, a craterlet, has manifested the greatest increase, while spot No. 19 has exhibited the largest decrease. The extent of variation of the separate spots is very irregular, and does not appear to indicate the operation of any general law. In one or two instances only have neighbouring spots been similarly affected." He concludes thus:—"The observations of the twelve lunations ending in March 1870, extend considerably the basis on which to found an intelligible explanation of the phenomena; it is nevertheless much too narrow to hazard more than conjecture. Another year's observations will doubtless throw further light on the subject."

Association between Sun-spots and Magnetic Declination Changes.—Prof. Wolf has instituted a comparison between recent changes in the condition of the sun's surface and the diurnal variations of the magnetic declination as observed at Christiania. It is worthy of notice that Professor Wolf had adopted an empirical formula for indicating the association between sun-spot periods and periods of declination or variation; so that the inquiry now instituted tends to determine how far what was in effect a prediction has been confirmed by the result. The calculated formula (from observations made in the years 1852–61) was for Christiania—

$$v = 0'{\cdot}0413r + 4'{\cdot}921$$

where v is the mean declination variation for the year, r the relative number indicating the frequency of sun-spots during the year. The following table indicates the calculated and the observed variations v and v' during the years 1864–69.

	1864	1865	1866	1867	1868	1869
v.	6′·87	6′·26	5′·64	5′·25	6′·58	8′·39
v'.	6′·00	5′·72	5′·70	5′·69	6′·65	7′·82

The accordance is close enough to indicate the truth of the general principles on which the Professor's formula is founded; but it is also clear that, with the progress of observation, the coefficients of the formula will have to be altered.

The Periods of Certain Meteoric Rings.—Professor Kirkwood has made another of his careful studies of meteoric and cometic phenomena. Comparing the period of the first comet of 1861 with the dates on which the most remarkable displays of the April meteoric shower have taken place, he shows that there are fatal objections to the hypothesis suggested by Dr. Weiss, that the comet and the meteors may be in some way associated. Continuing the inquiry, he shows further that the period of the meteors is probably about 28⅓ years, the major axis of their orbit about 18·59 (the earth's mean distance as unity). Thus the distance of the aphelion is nearly equal to the mean distance of Uranus. He notices that the meteors of December 11–13 seem to have a period of about 29⅛ years; those of October 15–21 a period of about 27½ years. He adds, "If these periods are correct, it is a remarkable coincidence that the aphelion distances of the meteoric rings of April 18–20, October 15–21, November 14, and December 11–13, as well as those of the comets 1866 I. and 1867 I. are all nearly equal to the mean distance of Uranus."

The Period of Algol.—Mr. Penrose has lately observed the epochs when this star has been at its minimum of brightness. His observations, which represent the phenomena certainly within 10 minutes, show that the period of 2·86727 days, which has been assigned to the variation of this remarkable star (combined with an epoch given for January 3, 1844), requires a slight correction; for the minima observed by Mr. Penrose occurred nearly three hours earlier than if due to those data. He assigns the period 2·867234. It may be that the change of period referred to by Sir John Herschel as actually in progress may account for the observed discrepancy.

The Planet Saturn.—Saturn is the only planet which will be well situated for observation during the ensuing quarter.

Total Lunar Eclipse.—There will be a total eclipse of the moon visible at Greenwich on the night of July 12, 1870. The following table gives the epochs of the principal phases.

		h.	m.
First contact with the penumbra	July 12	7	46·0 P.M.
First contact with the shadow .	„	8	44·6
Beginning of total phase . . .	„	9	44·4
Middle of the eclipse	„	10	34·2
End of total phase	„	11	24·0
Last contact with the shadow .	July 13	0	23·8 A.M.
Last contact with the penumbra .	„	1	22·4

At the middle of the eclipse the earth's shadow will extend beyond the totally eclipsed disc of the moon to a distance of 0·675, the moon's apparent diameter as unity.

BOTANY.

Parasitic Fungi on Lychnis diurna.—In one of the numbers of the *Gardener's Chronicle* for June, the Rev. M. J. Berkeley gives some results of his observations this year on the fungi which attack the anthers of *Lychnis diurna.* He says that among myriads of plants he has found a single plant only of the male and female form respectively, in which, while some blossoms were attacked, a few escaped. In most cases every flower was equally affected. In the male plant, and this only in the white-flowered variety, which is quite distinct from *Lychnis vespertina*, the unaffected flowers were in their normal condition, with scarcely a trace of pistil. In the female plant, though the pistil was partially developed, but in no case, as far as he could find, fertile, the stamens were uniformly developed in the affected flowers, and the anthers filled with the spores of the Ustilago. In the unaffected flowers there were just the same rudiments of stamens as are always visible in unaffected plants, and no further development. It seems perfectly clear, therefore, that, when the female plant is traversed by the mycelium of the fungus, there is a tendency to cause the development of the stamens, which takes place at the expense of the pistil, which is much reduced in size. There is not the slightest ground for considering it a reversion, and the true explanation is undoubtedly that, says Mr. Berkeley, which is given in the *Gardener's Chronicle* of last year [1869, p. 1,110]:—"The Ustilago penetrates the plant, but as it can only fructify in the stamens, it would appear to be the determining cause of the production of those organs in the normally female flower." Mr. Berkeley has planted affected female plants, and he says he shall be very surprised if any of them prove truly hermaphrodite, should they not be attacked by the fungus. Amongst thousands of unaffected plants he has not found a single one, after a long search, which combines both sexes.

The Alpine Flora of Britain.—In a paper lately read before the Geological Association of London, the Rev. James Crombie discusses the origin of the alpine (or boreal) flora of Great Britain, showing that it is directly due to a past geological condition of our island. This alpine flora is met with upon our higher mountains; but only on the higher ranges of Scotland, on the lofty and extensive Grampians, does it occur in great plenty and variety. The limits within which the plants are usually found are from about 1,506 to 4,406 feet (the summit of Ben Nevis). This space has been divided into certain zones, both physically and botanically distinct from each other, and these are applicable to all the loftier mountains, making due allowance for diversity of situation, mineralogical structure, &c. The flora of each is so similar in its main features as clearly to betray a common origin and a similar mode of distribution. Between our alpine flora and that of Scandinavia there is a great similarity, and the author considers that the boreal forms of Great Britain were derived from it. He agrees with the late Professor Edward Forbes, that the distribution of this alpine flora was effected during the Glacial epoch, which, indeed, the character of the plants themselves renders most probable.

The Chicago Botanical Society.—A Botanical Society, which promises to be most successful, is established at Chicago (U.S.).

Fungi on Insects.—The Academy gives an interesting abstract of a recent paper by Dr. Bail, of Dantzig, on this subject. *À propos* of the destruction of the finest trees by caterpillars, he states that in certain seasons these caterpillars appeared to be attacked by an epidemic, their bodies being swollen to bursting, and white threads being visible between the rings of the body, which seemed to issue from the body itself. In this condition great numbers were found still clinging to the leaves. The destroying agent had been identified by Dr. Reichhardt, of Vienna, as the mycelium of a fungus which he named *Empusa aulicæ*. The distribution of the *Empusa* is very considerable; the only order of insects which is not at present known to be subject to their attacks being the *Neuroptera* (dragon-flies, &c.); they are known to be parasitic upon *Coleoptera* (beetles), *Hymenoptera* (bees, ants, &c.), *Lepidoptera* (butterflies and moths), *Diptera* (flies and gnats), *Orthoptera* (crickets, &c.), and aphides, either in the larva or perfect condition, on water-insects, and even the same species on amphibia and fishes. Not only is their distribution over so many different animals remarkable, but also the prodigious rapidity of their development in the individual. The common house-fly is, in some years, destroyed by this parasite in vast numbers, and the dung-fly has been in certain districts almost annihilated. In the forests of Pomerania and Posen the caterpillars have been killed by it in such quantities that it may be considered to have saved the trees from total destruction. The fungi which Dr. Bail found to be the most destructive to insect life were those described by authors as *Cordyceps militaris*, *Isaria farinosa*, and *Penicillium glaucum*; the two latter forms he inclines to unite as different stages of growth of the same plant.

The Flowering and Fruiting of Aucuba Japonica.—At a meeting of the Botanical Society of Edinburgh, on April 14, Mr. P. S. Robertson stated that he had observed that recently-introduced female plants from Japan (grown in a coal-pit) came into flower in January and February, while the male plants, grown in the same circumstances, never came into flower till the middle of March. Yet he had every year obtained a crop of young seedlings from the berries, although the female flowers were quite shrivelled before the male ones expanded. He found that the common spotted variety, long grown in this country, did not flower till May or June, although grown in the pit or house with the others, and begins to expand its flowers when the males are getting past; yet it also never fails to produce a crop of fruit with perfect seeds. He thought that the pollen must lodge for some time in the scales of the unopened flower-buds, or reaches the pistils before the flowers are expanded; but how to account for the fertilizing of the early-flowering varieties he was at a loss. This year he had forced on the flowering of the male plants by placing them in strong heat, and had all the varieties of the male and female plants in full flower at very nearly the same time, and accordingly anticipates a much larger produce of berries than in former years, when they were left to the ordinary course. He exhibited a branch bearing berries with perfect seed; yet when that plant came into flower, there had not been a male plant in the house where it grew for fully a month previously.

The Medicinal Plants of India.—The *Indian Medical Gazette* says that the Secretary of State has authorised Mr. Broughton, the Government Quinolo-

gist at Madras, to investigate the properties of the indigenous medicinal plants of India. This is a most interesting and important branch of science —important to the people of India, whether regarded in a sanitary point of view, or as a means for developing the resources of the country—and we are very glad to see the Madras Government has given it the attention it merits. Mr. Broughton appears to have *carte blanche* given him in all arrangements connected with the observations, and the results are to be embodied in an annual report to the India Office.

Von Martius' Herbarium.—It is reported that this valuable collection has been obtained by the Belgian Government for 30,000 francs. It consists (1) of a collection of 60,000 species, including 300,000 specimens, nearly half of which are Brazilian; (2) of the great collection of palms; (3) a collection of fruits and seeds; (4) a series of woods; (5) a collection of drugs and economic specimens, in great part formed by his brother Theodore Martius, Professor of Pharmacy at Erlangen.

Destruction of Mould.—At the meeting of the Academy of Sciences, March 21, M. Dumas read a note by M. Raulin, describing some experiments on *Aspergillus niger*. These experiments tend to show that if this plant be placed in water in which are certain foreign substances, its vegetation becomes suspended. Thus, to completely destroy the development of a mould, it suffices to introduce into the water which nourishes it 0·00016 part of nitrate of silver, 0.00006 of corrosive sublimate, 0·008 of chloride of platinum, 0·02 of sulphate of copper.

Fertilization of Plants.—Professor Hildebrand (says the *American Naturalist*) states that plants intermediate between the Papaveraceæ and the Fumariæ gave the greatest quantity of seeds when impregnated with the pollen of another individual of the same species; less when the pollen was taken from another flower of the same individual, and least when the impregnation took place within the flower itself. For *Eschsholtzia Californica*, the proportion of seeds in these three cases was as twenty-four to nine to six. Professor Fewzl says that he obtained abundance of seeds from two species of Abutilon by fecundation with pollen from other individuals, and that these operations are best performed between eight and nine A.M.

The Production of New Species among Plants.—In the Philosophical address delivered before the Linnean Society on May 24, Mr. G. Bentham, F.R.S., made the following highly-important and suggestive remarks relative to the formation of new species of plants:—Taking into consideration the new lights that have been thrown upon these subjects by the above investigations, and by the numerous observations called forth by the development of the great Darwinian theories, amongst which I may include a few points adverted to in a paper on Cassia which I laid before you last year, it appears to me that in plants, at least, we may almost watch, as it were, the process of specific change actually going on, or at least we may observe different races now living in different stages of progress, from the slight local variation to the distinct species and genus. As a first step we may take, for instance, those races which are regarded by the majority of botanists as very variable species, such as *Rubus fruticosus, Rosa canina, Zornia diphylla, Cassia mimosoides*, &c.; we shall find in each some one form, which we call typical, generally prevalent over the greater part of the area of the race,

whilst others more or less aberrant are more or less restricted to particular localities, the same varieties not occurring in disconnected stations with precisely the same combinations of character; and in the same proportions local and representative varieties and sub-species are being formed, but have not yet obtained sufficient advantages to prevent their being kept in check by their inter-communication (and probable cross-breeding) with their more robust type. The British rubologist or rhodologist transported to the south of France or to Hungary will still find one, or perhaps two or three, forms of bramble and dog-rose with which he is familiar; but if he wishes to discriminate the thirty or forty varieties or sub-species upon which he has spent so much labour and acuteness at home, he will find that he must recommence with a series of forms and combinations of characters quite new to him. The species is still the same; the varieties are changed.

A Pomological Dictionary.—The *Athenæum* states that M. André Leroy, of Antwerp, is engaged on a Dictionary of Pomology: three large volumes are ready, and treat of pears, apples, quinces, service-trees, and medlars. Two more volumes will complete the work, one of which will treat of stone-fruits, the other of grapes and miscellaneous fruits. Each species of fruit is treated in an elaborate way, and to the mode of its culture is prefixed a history of its culture (besides several types of each variety—915 varieties of pears are described), and each description is accompanied by a woodcut.

Double-Flowers and Variegations.—M. Morren, of Liége, has established the fact that (contrary to a supposed law) double-flowers and variegations can occur together on the same plant.

CHEMISTRY.

The Detection of Logwood Colour in Wine.—This is an important application of chemistry to hygiene, and may interest some of our readers. In the *Journal de Pharmacie* for April, M. Lapeyrère states that, while studying some of the properties of the colouring principle of logwood, he found that the hematine it contains yields a sky-blue colour with salts of copper. In order to apply this test to wines for detecting if they are doctored with logwood, it is only necessary to place strips of good filtering-paper, Swedish being preferred, into an aqueous solution of neutral acetate of copper, and, after drying, use one of these slips to test the wine suspected to be adulterated with logwood colour, by dipping the paper into the wine; and, on removing it from that fluid, care should be taken to cause the adhering drop of wine to flow backwards and forwards over the paper, which is next rapidly but carefully dried. If the wine be as it naturally ought to be, the colour exhibited after drying will be grey, or rose-red greyish; but, if logwood is present, the tinge will be distinctly sky-blue.

The Homologues of Schweinfurt Green.—In a short paper, communicated by the *Chemical News* (June 10), Mr. P. S. Strahan, B.Sc., says he has recently found that, if cupric formiate, butyrate, or valerianate be substituted for cupric acetate in the process for the manufacture of Schweinfurt

green, compounds are obtained similar in colour to the well-known green, but containing formic, butyric, or valerianic acid in place of the acetic. For the preparation of cupric formo-arsenite, four parts of cupric formiate were dissolved in as little water as possible, and added to four parts of arsenious acid, dissolved in about fifty parts of water by the aid of a little caustic soda; both solutions being at the boiling-point. The yellowish precipitate which appeared soon became of a very bright green, with a slightly more yellowish tinge than the acetic compound. The butyric and valerianic greens were made in a similar manner, but with different proportions of cupric salt and arsenious acid.

Hydrogen and Mercury Amalgam.—In a late number of the *Scientific American* Dr. Loew has a paper, describing the experiments of others and his own observations on this preparation. He thinks that Schoenbein, in his search for ozone, found a method for making the peroxide of hydrogen, which brought him to the very threshold of discovering hydrogenium. Schoenbein's experiment was this—An amalgam of zinc and mercury is violently agitated in water; the water is then filtered, and, on being examined with iodide of starch and protosulphate of iron, will be found to contain peroxide of hydrogen or oxygenated water. Dr. Loew has carried the investigaion further, and has, instead of oxidising the hydrogen, succeeded in combining it with the mercury. He takes an amalgam composed of not more than three or four per cent. of zinc, and shakes it with a solution of bichloride of platinum; the liquid becomes black, and a dark powder settles to the bottom. The contents of the flask are then thrown into water, and hydrochloric acid added to dissolve the excess of zinc. The amalgam of hydrogen and mercury at once forms a brilliant voluminous mass, resembling in every way the well-known ammonium amalgam. It is soft and spongy, and rapidly decomposes, but without any smell of ammonia. The hydrogen escapes, and soon nothing but pure mercury is left in the dish. The experiment appears to show conclusively that an amalgam of hydrogen and mercury can be formed, and that hydrogen is really a metal. It would also throw some doubt upon the existence of the amalgam of ammonium and mercury, and offer an explanation of that compound on the basis of its being the same amalgam of hydrogen and mercury that is prepared in the way now pointed out by Dr. Loew. The smell of escaping ammonia must be traced to some other source than the existence of that radical in combination with mercury.

A Bell-jar and Beaker with Bunsen's Pump.—Mr. Albert R. Leeds, writing to the *Journal of the Franklin Institute,* gives the following account of this contrivance. Instead of the cumbrous bottle generally employed, the plate of an air-pump may be advantageously substituted, and a receiver, with an India-rubber cork, through which the neck of the funnel is passed. In order to prevent loss by the bursting of bubbles of air at the end of the tube of the funnel, it is continued low down into the beaker by an India-rubber tube. The latter can easily be removed at the end of a filtration, and the drops of adhering liquid washed with water from the spritz into the filtrate. This simple arrangement obviates the necessity of a transfer from one vessel to another, and permits all the operations of an analysis to be carried on as usual, in beakers. Instead of an air-pump plate, a sheet of

glass may be used, and the exhausting tube as well as the funnel in this case must be passed through the tubulure of the bell-jar. A similar arrangement is useful for the desiccation of bodies, either alone or with sulphuric acid.

How to Demonstrate the Increase of Weight of Burning Bodies.—The *Chemical News*, abstracting a paper lately read before the Chemical Society of Berlin, says that a very striking mode of demonstrating in the lecture-room that burning bodies increase in weight has been contrived by H. Kolbe. A glass rod is fastened, in a horizontal position, to one arm of a balance. Upon this is fastened a glass cylinder in which a candle is burnt, connected with which, by a glass tube, there is a V-tube for condensing the vapour, a flask filled with lime-water for carbonic anhydride, and two more U-tubes containing soda-lime. The last are connected, by an India-rubber tube, with a Bunsen's pump, by which a steady current of air is drawn through the apparatus. The beam is first counterpoised; as the candle burns away, the arm of the balance to which it is attached sinks down until its progress is arrested by the table.

The Literature of Alizarine.—In his interesting lecture before the Royal Institution, Professor Roscoe gives the following list of the contributions to the history of Alizarine: and, as it may be useful for purposes of reference, we reproduce it:—

CONTRIBUTIONS TO THE HISTORY OF ALIZARINE. $C_{14} H_8 O_4$.

1825. Faraday discovered Benzol in Coal-gas Oil. $C_6 H_6$.
1831. Robiquet and Colin discovered Alizarine in Madder Root.
1832. Dumas and Laurent discovered Anthracene in Coal Oils.
1848. Schunck gave the Composition of Alizarine. $C_{14} H_{10} O_4$.
1850. Strecker " " " $C_{10} H_6 O_3$.
1862. Anderson examined Anthracene Compounds. $C_{14} H_{10}$.
1865. Kekulé explained the constitution of the Aromatic Compounds.
1866. Baeyer obtained Benzol from Phenol.
1868. Graebe investigated the Quinones.
1868. Graebe and Liebermann obtained Anthracene from Alizarine.
1869. " " " Alizarine from Anthracene.

The Reactions of Aldehyde.—In a paper read before the Chemical Society of Berlin, Herren Krämer and Pinner describe the results of their researches on aldehyde by submitting it to the action of chlorine-gas. Conducted in this way, the reaction takes place in a different manner from that described by Wurtz, who, pouring an excess of aldehyde into large vessels filled with chlorine, obtained chloride of acetyle and its compound with aldehyde. Neither of these substances has been obtained by Messrs. Krämer and Pinner. Nor is ordinary chloral obtained by this reaction, the aldehyde being entirely converted into the chloral of the condensed aldehyde, C_4H_6O, known as crotonic aldehyde. Crotonic chloral is a liquid, boiling at 165°, and forming with water, but not with alcohol, a crystalline compound. By oxydation it forms trichlorocrotonic acid. Caustic potash transforms it into the corresponding chloroform $C_3H_3Cl_3$ and its derivative $C_3H_2Cl_2$ (bichlorinated allylene?) boiling at 78°.

The Chemistry of Vanadium.—In his communication to the Chemical

Society recently, Professor Roscoe gave an elaborate account of his investigations into the chemistry of this element. Professor Roscoe, it seems, has proved that the substance supposed by Berzelius to be vanadium, is not the metal, but an oxide, and that the true atomic weight of the metal is 51·3. The vanadic acid, VO_3, of Berzelius, hence, becomes V_2O_5, corresponding to P_2O_5 and As_2O_5; and the above-mentioned isomorphism is fully explained. The suboxide of Berzelius is a tri-oxide, V_2O_3; whilst the terchloride (VCl_3) of Berzelius is an oxychloride, $VOCl_3$, corresponding to oxychloride of phosphorus, $POCl_3$. Professor Roscoe has succeeded in obtaining bromine and iodine compounds of vanadium, and also various metallic vanadates. In the course of his lecture he pointed out that the characters of the vanadates bear out the analogy of the vanadic acid with the highest oxides of phosphorus and arsenic; and stated, in conclusion, that vanadium, hitherto standing in no definite relation to other elements, must now be regarded as a member of the well-known triad class of elementary substances, comprising nitrogen, phosphorus, boron, arsenic, antimony, and bismuth.

Transformation of Isatine into Indigo.—At the meeting of the Chemical Society of Berlin on May 23, Herren Baeyer and Emmerling reported their researches on the transformation of isatine into indigo. As isatine, when treated with nascent hydrogen, unites with it and forms indol, a substance not capable of uniting with the reduced substance was sought for, and discovered in phosphorus, the solvent employed being chloride of acetyle or of phosphorus. Real indigo-blue and indigo-red were thus produced. The latter stands in the same relation to the blue as purpurine does to alizarine. To complete the long hoped-for discovery of producing artificial indigo, all that remains to be done is now to transform indol into isatine.

The Chemistry of Hair-dyes.—Mr. G. McDonald, writing to the *American Journal of Pharmacy* (May), on the subject of a brown hair-dye, gives some chemical hints of considerable practical interest. Mr. McDonald says, the well-known fact that a soluble compound of lead and sulphur could not be obtained by the decomposition of a soluble lead salt by a soluble sulphuret, or, in other words, the insolubility of the sulphuret of lead was regarded as an indubitable proof of the folly of undertaking to search for a compound containing sulphuret of lead in a soluble state, and yet so as to be innocuous to the system. There is a class of salts known as hyposulphites, many of which are freely soluble in water, and which are readily converted by absorption of oxygen into sulphate of the base and free sulphur; it is in the use of these salts that the key to the enigma lies. Chemical text-books state that hyposulphite of lead is insoluble in water, which is quite correct; but, like many other precipitates insoluble in water, it is readily dissolved by an excess of the precipitant; thus, if we add to a solution of *three* parts of acetate of lead *two* parts of hyposulphite of soda, we shall have a curdy white precipitate of hyposulphite of lead, insoluble in water; but if we add to this *ten* parts more of hyposulphite of soda, the precipitate will be redissolved, and a perfectly clear solution will be the result; this solution, when applied to the hair, is decomposed by absorption of oxygen; one of the results thereof is the formation of the dark brown sulphuret of lead; it is to the formation of this compound in the hair that all lead and sulphur dyes owe their efficacy.

GEOLOGY AND PALÆONTOLOGY.

The Fossil Horse in Missouri.—In a paper read before the St. Louis Academy of Science, and reported in the *American Naturalist* for March, Mr. G. C. Broadhead records some interesting facts on the above subject. Alluding to the fact that horse remains had been found in the altered drift of Kansas, he says he is now able to announce that similar remains have recently been discovered in a well at Papinville, Bates County, Missouri. Mr. O. P. Ohlinger procured a tooth at the depth of thirty-one feet from the surface, resting in a bed of sand beneath a four-inch stratum of bluish clay and gravel. Above the last was thirty feet ten inches of yellowish clay reaching to the surface. Beneath the sand, containing the tooth, was a gravel bed five feet in thickness, consisting mostly of rounded pebbles resembling river gravel. generally hornstone, many partially, and some firmly adhering together. Other pebbles shown him from the same bed were of iron ore, coal and micaceous sandstone. He was farther informed that some remains of fluviatile shells were found. He sent the tooth to Professor Joseph Leidy of Philadelphia, who pronounced it to be the last upper molar of a horse, probably an extinct species. From a similar gravel bed on the banks of Marais des Cygnes, a fragment of a tusk was given him resembling very much that of a mammoth. Its whole length was said to be seven feet four inches. About ten miles above Papinville, the banks of Marais des Cygnes River appear to be of a similar formation to the well of Ohlinger, consisting of about twelve feet of brown sandy clay resting on ten feet of blue clay with many pebbles of worn gravel at the lower part. These gravel beds he considers as of more recent age than the drift, but older than the bluff or loess, and regards them as altered drift. They seem rather to abound on the Osage and its tributaries, and are often reached in digging wells.

New Mosasauroid Reptiles.—Professor Marsh has recently published in the *American Journal of Science*, a notice of four new reptiles, belonging, or allied, to Mosasaurus, from the Greensand of New Jersey. He remarks that "a striking difference between the reptilian fauna of the Cretaceous of Europe and America is the prevalence, in the former, of remains of Ichthyosaurus and Plesiosaurus, which here appear to be entirely wanting; while the Mosasauroids, a group comparatively rare in the Old World, replace them in this country, and are abundantly represented by several genera and numerous species.

The Geology of the Missouri.—We have received a very important geological work, entitled *Geological Report of the Exploration of the Yellowstone and Missouri Rivers*, by Dr. F. V. Hayden (Washington, 1869). The work is one well worthy of the careful attention of geologists. In speaking of the Rocky Mountains, the author describes two types: (1) Those with a granite nucleus and regular outline, and (2) those composed of erupted rocks, rugged in their outlines and irregular in their trend. Several good maps and sections are appended to the volume.

Antiquity of Man in North America.—In the Transactions of the Chicago Academy of Sciences (vol. i. part 2) Dr. J. W. Foster contributes a paper of some interest relative to the above. He gives man a great antiquity, and refers to the discovery in California by Professor Whitney of a human skull

buried deep in the gold drift, and covered with five successive overflows of lava.

The Cetiosaurus, or Great Oolitic Lizard.—The *Geological Magazine* of July 1869 announced the discovery of the thigh-bone of this monster, and since then Professor Phillips, writing to the *Athenæum* of April 2, states that further remains have been found. "The space of ground in which the bones are found (writes Prof. Phillips) is apparently quite limited. One may think the whole body of the vast old lizard, in the extremity of age, was here laid to uneasy rest; the parts separated by decay; the massive limbs disjointed, and the bones displaced. Imagine a surface of the ossiferous clay which covers the Oolite laid bare by the workmen. Look southward: before you are four bones laid rudely parallel, in a row, at intervals of 1, 2, or 3 feet. They are 64, 54, 45, and 37 inches long; 10 inches the least breadth in the narrowest part; 26 inches the greatest breadth in the widest part. These are bones of *Cetiosaurus*. Over them and in front of them, three days since, lay as many others, as large and as quietly reposing in their 'longæval' graves; behind them, possibly, are still more bones, to be discovered at some future time. Bones of a much mightier area—probably hugest of all huge ilia—extended far and wide; vertebræ 8, 9, and 11 inches in diameter; monstrous ribs, of which the parts traceable and inferred are 59 inches long; all this within the compass of a few square yards. It seems like the burial-place of the great father of lizards, each of whose bones demanded—but only some could obtain—a separate grave."

Fossil Insects in the Bournemouth Leaf-bed.—The following letter, which appears in the *Geological Magazine* for May, may interest some of our readers. It is from Mr. J. F. Walker:—The Rev. P. B. Brodie, in the March number of the *Geological Magazine*, p. 141, directs the attention of the explorers of the Leaf-beds in the Lower Bagshot Series of Hants and Dorset, to the desirability of looking out for insect-remains; particularly at Bournemouth. Mr. Brodie may be glad to learn that Mr. Wanklyn has already recorded the discovery of insect-remains at Bournemouth, in the *Annals and Magazine of Natural History* for January 1869, 4th series, vol. iii., No. 13, p. 10. The specimen has been placed by the discoverer in the hands of Mr. W. S. Dallas, F.L.S., the Assistant Secretary of the Geological Society of London, who has kindly undertaken to examine and describe it.

Eminent Living Geologists.—The *Geological Magazine* has shown considerable enterprise in publishing, as it has done recently, a series of biographical sketches as above. Those which have already appeared are biographies of Sedgwick and Scrope, and are accompanied by excellent engravings of these well-known workers in the field of Geology.

The Milk-dentition of Palæotherium magnum.—Professor Huxley contributes a brief but very important paper on this subject to the *Geological Magazine* for April. A plate accompanies the paper. The author deals with some serious errors which a certain well-known palæontologist made some time ago. He shows the mistake on which the genus *Palaplotherium* was founded, and by which M. Pictet was misled.

The Cheshire Sands.—Mr. J. E. Taylor, in a brief communication, gives the following as a list of beds passed through in the sinking of a well for

cottage purposes. The thinness of some of the beds is indicative of their shallow-water origin, as well as perhaps of their local extension. But as this is a feature in the Middle Drift sands, as far as he has been an observer, it is not worth while to state more than that he believes the following to be a good typical section of the Cheshire sands;—

	FEET
Sand (current-bedded)	21
Fine Gravel (with shells)	3
Ochreous Sand	3
Shelly Gravel	3
Fine Clay	8
Stiff Clay, with pebbles and shells	30
Brick Earth	8
Total	76

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Fossil Botany in 1869.—Mr. W. Carruthers gives an excellent list of the memoirs published in 1869. It appeared originally in the *Journal of Botany*, but has been reprinted in the *Geological Magazine* for April.

The Suffolk Bone-bed and the Norfolk Stone-bed.—At the meeting of the Geological Society on May 25 Professor Huxley presented a paper on this point by Mr. E. Ray Lankester. The author, whose paper elicited much discussion, pointed out that the recognition of the distinction of these two deposits from the overlying shelly crags was an important step in the determination of the history of these beds. He combated the notion that the Bone-bed and Stone-bed were identical in their contents; and especially dwelt on the differences of the mammalian fauna found in the two. The late Dr. Falconer's views, hitherto prevalent, consisting in regarding the fauna of the Suffolk Bone-bed, Norfolk Stone-bed, and Forest-bed as all of one and the same history and extent, he most strongly opposed. *Rhinoceros Schleiermacheri*, *Tapirus priscus*, Hipparion, *Hyæna antiqua*, and a well-defined Miocene Mastodon (Fauna 1), had been found in the Bone-bed below the Suffolk Crag; the first three in some abundance, but never in the Stone-bed or Forest-bed of Norfolk. They belonged to a different fauna from that indicated by the other mammals common to the Bone-bed and Stone-bed (Fauna 2), viz. *Mastodon arvernensis*, Equus sp., and certain forms of Cervus (studied by Mr. Boyd Dawkins). On the other hand, the *Elephas meridionalis* (Fauna 3), occurring in the Norfolk Stone-bed and in the Forest-bed, had *never* been found in the Suffolk Bone-bed. Mr. Lankester suggested that the association of the first two of these three groups of mammals in Suffolk, and of the second two in Norfolk, might be explained by the hypothesis that they succeeded one another in time, the first (late Miocene) being confined to Suffolk, and dating from before the Diestien period, since he had obtained a Mastodon tooth of the *M. tapiroides* form enclosed in a Diestien box-stone, the third having existed in Norfolk at a period subsequent to the Coralline Crag, but before the Norwich Crag was deposited, chiefly represented in the Lower part of the Forest-bed, but also in the Stone-bed, whilst the second group of mammals had existed in both areas at an intermediate period. Mr. Lankester maintained that this was

the explanation suited to the facts as they at present stand, and considered that the question was not one to be shirked. All geological inferences from palæontology rest on what is called negative evidence, and hypotheses must be used in investigation. It was shown that the London clay had contributed very little indeed to the lot of mammalian remains found in the Suffolk Bone-bed. Six teeth of Coryphodon and four fragments of Hyracotherium were all that could be found in the various collections.

MECHANICAL SCIENCE.

Railway Carriage Coupling.—Mr. J. Mackenzie has described to the Scottish Society of Arts a new coupling for railway carriages, designed to obviate the dangers which attend the process of coupling and uncoupling on the present system, especially with goods-trains. It is simple, and the waggon fitted with it may be coupled to other waggons not so fitted. Hence it may be gradually introduced.

Scoop-Wheels.—Mr. W. Airy has investigated the sources of waste of power in the scoop-wheels employed in draining the Lincolnshire fens, and has made some suggestions for the improvement of their construction. Mr. Airy's papers will be found in *Engineering*, vol. ix. pp. 194, 230, 274, and 321, and are well worthy of the attention of those engaged in extensive drainage operations.

Torpedoes.—Captain Harvey's towing torpedo, on which very successful experiments have been made, has, it appears, been adopted by the Government. This torpedo acts on the principle of the "*otter*" sometimes used in fishing, or, in other words, is a sort of water-kite, carrying a torpedo, and towed by the attacking vessel. Captain Harvey has been appointed to instruct the officers and men on board H.M.'s gunnery ships "Excellent" and "Cambridge" in the use of the torpedo.

Meeting of the Institute of Naval Architects.—At the last meeting of this Institute, perhaps the paper of most general interest was one by Mr. Scott Russell on Channel Communication, in which he applied his experience gained in the construction of the steam-ferry on Lake Constance to solving the problem of the Dover and Calais passage. Mr. Samuda read an interesting paper on the Influence of the Suez Canal on Ocean Navigation. M. Emile Leclert sent a paper on Certain Theorems respecting the Geometry of Ships, in which he discusses the surface which is the envelope of the planes which cut off displacements of a constant volume; and the surface which is the locus of the centres of buoyancy of these volumes of equal displacement. Sir E. Belcher read a paper on the Channel Passage; Professor Macquorn Rankine, a paper on Stream Lines. Sir W. Fairbairn sent an interesting communication detailing further experiments on the Law of Penetration of Armour-plates by Projectiles. Mr. Arthur Rigg contributed a paper on Compound Marine Steam-Engines.

Experiments on the Resistance of Ships.—Our readers may recollect that a Committee of the British Association reported in favour of a series of

experiments on the resistance of full-sized ships, and memorialised the Admiralty to afford the use of one of Her Majesty's vessels for the purposes of the experiments. Mr. Froude had previously applied to the Admiralty for assistance to carry out experiments on small models of ships on a plan which will, in his opinion, afford trustworthy data for calculating the resistance and for determining the proper form of ships. The Admiralty have decided to adopt Mr. Froude's proposal. It is to be hoped, however, that some experiments on the resistance of actual ships will not be long postponed. Valuable as Mr. Froude's experiments will undoubtedly be, naval architects and engineers require the data which would be derived from full-scale experiments in addition.

Victoria Stone.—*Engineering* for May 23 describes a new artificial stone which is being manufactured near Victoria Park, by a process perfected by the Rev. H. Highton. The process consists of mixing broken granite, the refuse of the granite quarries, with hydraulic cement, and steeping the mass in silicate of soda. The most novel part of the process is in the way in which the silicate of soda is applied. The concrete masses which are to be silicified are immersed in a tank of silicate of soda, in which are placed pieces of a siliceous stone obtained at Farnham, which has the peculiarity that the silica is in such a state that it dissolves in cold caustic soda. The lime of the concrete mass takes silica from the silicate, and the soda set free redissolves silica from the Farnham stone; hence the process is continuous.

MEDICAL SCIENCE.

Influence of Magnets on Animals.—In the April number of the *American Psychological Journal* there is an extremely interesting paper by Dr. John Vansant, describing the results of several curious experiments with magnets on animals and plants. Dr. Vansant's attention was first particularly directed to the subject of this communication in the winter of 1866, when he observed that a small magnetized steel rod, the *ends of which were finely pointed*, if brought carefully in contact with an exquisitely sensitive blister, that had been accidentally produced on one of his fingers by pinching it, gave rise, when the southward pole was applied, to a momentary sharp sensation, and seemed to cause the blister to be more painful after the magnet was removed. When the northward pole was used in the same way, no sensation could be felt at the moment of contact, and after removal the original pain remarkably subsided. Struck by this phenomenon, and yet almost doubting his own sensations, he proceeded to inquire if it were possible to recognize a difference between the two ends of the magnet by means of some organ peculiarly sensitive in its normal condition. On trial, he found that the conjunctival membrane of the eye would indicate by the feeling which pole it was touched with. He could lay with care the sharpened northward end of the magnetic rod on that membrane without pain or winking; but the instant the southward pole was applied, no matter with how much delicacy, there was a sharp sensation and an involuntary slight closing of the eyelid. The effect was faint, but plain. This experiment

was repeated on the eye of another person, the same day, with similar results. After this he made numerous experiments with magnets of different forms and powers (though not at any time with very large ones) applied to various parts of the body, and thought he observed a definite set of symptoms arise after every application in the same manner to a given part, provided sufficient time had elapsed between the applications. The author then details several curious experiments, which some of our readers would do well to repeat.

Dust and the Germ Theory of Disease.—The Scientific American reports that the New York Officers of Health have been examining the dust of the air in the American city. Over one hundred specimens of the particles floating in the air and falling as dust were collected on plates of glass, and were examined under the microscope. The proportions of the different ingredients varied, but the same substances were found in all the specimens. The composition of the matter subjected to the microscope was as follows:— "The dust of the streets, in its finer or coarser particles, according to the height at which it had been collected, with a large proportion of organic elements; particles of sand, of quartz, and felspar; of carbon, from coal-dust, and lampblack; fibres of wool and cotton of various tints; epidermic scales; granules of starch, of wheat, mainly the tissues of plants; the epidermic tissue, recognised by the stomata, or breathing pores; vegetable ducts and fibres, with spiral markings; vegetable hairs or down, either single or in tufts of four or eight, and of great variety, and three distinct kinds of pollens. Fungi were abundant, from mere micrococcus granules to filaments of mould. When water was added to a portion of dust, from whatever source, and exposed in a test-tube to sunlight or heat for a few hours, vibriones and bacteria made their appearance."

Diseases of Lucifer-Match Makers.—At the meeting of the Academy of Sciences of Paris on March 21 M. Jouglet read a paper on the diseases of the above class. He states that these people suffer from a peculiar series of diseases of the reproductive system.

Functions of the Nerve-Centres in the Frog.—The *Academy* reports that Professor F. Goltz, of Königsberg, has been continuing his observations on the different nerve-centres of the frog. After removing the cerebrum with as little effusion of blood as possible, the frog remained on the table in exactly the position of a sound animal, and without any indication of the injury it had sustained; but, of its own accord, would never change the position once assumed. If pinched or pressed, it would turn itself round, or remove itself by a leap from the external pressure, but would then remain equally unchangeable in its new attitude.

Influence of the Sewing-Machine on Health.—Dr. Decaisne's report on this subject to the French Academy (May 16) is somewhat reassuring. Concerning the influence of the machine on the nervous system, he does not consider the noise of the machine injurious in any way, as has been alleged; for it is acknowledged by all workwomen that, although at first the vibration of the machine occasions them some inconvenience, they soon become accustomed to it, and also that it has no effect whatever on their health. Dr. Decaisne is of opinion that the sewing-machine, as worked by women, when employed within reasonable limits, is not more injurious for the health than

work at the needle. That which proves it is this: that amongst twenty-eight women, from eighteen to forty years of age, working from three to four hours a day at the sewing-machine, it was impossible to find any disorder which might be attributed to its use. M. Decaisne, by the conclusions which he has drawn from a close observation of the use of the sewing-machine, refuted some of the objections made with regard to it; but he overlooked altogether the gravest side of the question, which is, that most of the women employed at those machines work, not from two to three hours, but from twelve to fifteen hours a day; for it is only by this excessive work that sewing-machines become remunerative to them and their employers. Dr. Decaisne thinks that in selecting a machine it is best to employ those in which the pedals are worked isochronously, and not alternately.

The New Prælector in Physiology at Cambridge.—Dr. Michael Foster has been elected to this post at Trinity College, Cambridge. Scientific men will congratulate both Dr. Foster and the University on the selection made.

A New Remedy for Intermittent Fever.—The *Wiener Medizinische Wochenschrift* of May 14 publishes, says the *British Medical Journal*, the results of a number of observations made regarding the effect of a new remedy for intermittent fever. The remedy is the tincture of the leaves of the *Eucalyptus globulus*, a plant of the natural order *Myrtaceæ*. In 1869 Dr. Lorinser made some experiments, the results of which he published: but he was brought to a standstill by the want of a supply of the medicine. The plant has since been cultivated by Herr Lamatsch, an apothecary; and a sufficient quantity of tincture has been made from the leaves to supply a number of medical men in the districts of the Theiss and the Danube, and in the Banat. The records of fifty-three cases of intermittent fever in which the Eucalyptus was administered have been communicated to Dr. Lorinser.

The Chair of Physiology in Prague has been filled up by the election of Herr Hering, the well-known investigator of the liver. It had been previously held by the late distinguished biologist M. Purkinje.

American Opium.—*The Journal of Applied Chemistry*, in a recent number, states that Mr. C. Wilson, of Monkton, Vermont, sowed, in the spring of 1868, rather more than six and a quarter acres with opium poppy seed. The yield from the gathered juice of the poppy heads or capsules was 640 pounds, which, when dried, became marketable opium. For this the grower obtained prices ranging from $8 to $10 per pound, from druggists and physicians in New England. The opium furnished 6·25 per cent. of morphine. It is stated that Professor Proctor thinks, with greater care in obtaining the pure juice of the capsules, the opium might be made to yield ten per cent. of morphia. The proportion of this alkaloid which the best Turkey opium is capable of affording varies from nine to fourteen per cent.

The late Sir James Y. Simpson.—Medicine has sustained a heavy loss during the quarter in the death of the celebrated Northern physician and archæologist who introduced chloroform into practice.

Artificial Teeth.—The following account of the mode of making teeth is from one of the United States' scientific journals, and it may suggest something to those interested in such matters:—"It is stated that at least 3,000,000 of teeth are annually made in this country alone. The first operation, according to the method of manufacture pursued at one of the

most extensive and celebrated establishments, is the choosing of the materials. These are felspar, silica, and clay. To these are added various metallic oxides to produce any shade of colour desired. The felspar, clay, &c., are ground to an impalpable powder under water, dried, and made into a paste. That composing the body of the tooth is of different materials from that composing the base or enamel. The teeth are made in brass moulds, and this is quite a delicate process. The enamel is first put in place with a small steel spatula; the platinum rivets, by which the teeth are fastened to the plate, are placed in position, and then the body is pressed into the mould. They are then submitted to powerful pressure and dried. After being dried, they are submitted to a process called biscuiting, in which they can be cut like chalk. They are then sent to the trimmers, who scrape off all projections, and fill up all depressions which may have been left in the operation of moulding, and then wash them with what is technically termed enamel. This is composed of various substances, more fusible than the tooth itself, and answers the glaze in common porcelain making. It is ground to a fine powder, and suspended in water, and is laid on with a camel-hair brush. They are now sent to the gummers, who apply the gum. This is chiefly composed of oxide of gold, and is applied in the same manner as the enamel. After being dried, they are burned. This operation is carried on in a muffle. The teeth are placed on a bed of crushed quartz, which is placed on a slab of refractory clay. After being exposed to an intense heat for some hours, they are taken out, cooled, and assorted."

The New (?) Anæsthetic.—It has been stated in various medical and scientific journals that Dr. Liebreich has discovered a new anæsthetic—Chloride of Ethylene. In point of fact, we believe that this is no new discovery; for this substance was employed even by Dr. Snow in some of his inquiries. Its merits too appear to have been much overrated.

A New Theory of Nervous Sensation.—The *Chemical News* (June 3) gives a report of a recent meeting of the Royal Irish Academy at which a paper was read by Dr. Robert M'Donnell on the above subject. The author's paper might be briefly described as an application of a theory similar to the wave theory of light to the propagation of sensation along the nerves. He compared this "undulatory" theory of nervous conduction with the hitherto more generally received hypothesis of distinct nerve-conductors, supposed to exist for each kind of sensation, pain, heat, tickling, &c.; and attempted to point out that the former is at once a simple hypothesis, and more in harmony with the ideas now prevalent as to the propagation of light, heat, electricity, &c. The author also dwelt upon many points of analogy between the absorption or interception of waves of heat or of light, and the somewhat similar phenomena as regards nerve-conduction where one kind of sensation is felt and another ceases to be any longer perceived, as in cases where the patient feels the contact of the hand, but cannot distinguish heat, or *vice versâ.*

Action of Alcohol on the Body.—A paper was read before the Royal Society in May by Professor Parkes and the Count Wollowicz, M.D., detailing their experiments on the action of ordinary alcohol on the human body. Among the results were the following:—Small quantities of absolute alcohol (1 and 2 fluid ounces = respectively to 28·4 and 56·8 c.c.), given

in divided doses to a perfectly healthy man, seemed to increase his appetite; 4 fluid ounces lessened it considerably, and larger quantities almost destroyed it. In other healthy persons it may be different from the above, while in most cases of disease it seems probable that a much smaller amount of alcohol would destroy appetite. The number of beats of the heart in twenty-four hours (as calculated from eight observations made in fourteen hours) increases very largely—viz. an average of more than 13 per cent.; while the actual work done by the heart in excess was found to be equal to lifting 15·8 tons 1 foot, and during the two last days of the experiment it did extra work to the amount of 24 tons lifted as far.

METALLURGY, MINERALOGY, AND MINING.

The Combustion of the Diamond.—Professor Morren of Marseilles recently laid a paper before the French Academy (May 2) on the combustion of the diamond in various gases. When the diamond has been heated in a platina tube, through which some hydro-carburetted gas has passed, the diamond becomes covered with an adhering black layer, which does not alter the diamond, and can easily be removed by a simple elevation of the temperature. This elevation should not be carried too far, however, as it would change the polish of the stone and its brilliancy; but if, instead of such a gas as the above, pure dry hydrogen is passed through the tube, the diamond may be submitted to a higher temperature without in any way altering its appearance. If carbonic acid be used, this gas is decomposed, and oxygen and carbonic oxide are formed in the tube. But this decomposition takes place when there is no diamond in the tube, and is, therefore, merely an effect of the temperature. When the diamond is heated in oxygen, it burns, being surrounded by a sort of halo; and after a certain time it completely disappears. If the diamond be examined with a microscope before it has entirely disappeared, it is seen that the faces of the crystal, instead of being a plane, are covered with a number of small elementary facettes.

Composition of Chabacite.—Herr Kengott gives an account of this in the *Journal für prakt. Chem.* (Nos. 2 and 3). He gives, first, a review of the various results of analysis of the mineral called chabacite, as obtained by different authors, and next, a lengthy discussion as to the proper formula to be assigned to this mineral, because, although it has been analysed by the most competent hands, and the mineral is crystalline, there are great discrepancies in the results. Chabacite contains, in 100 parts, according to some analysts, silica, 51·46; alumina, 17·65; lime, 8·91; soda, 1·09; potassa, 0·17; water, 19·66; peroxide of iron, 0·85.

Some Chili Minerals.—The *Chemical News*, in a recent number, gives an abstract of a paper by Mr. Domeyko in the *Annales des Mines* (No. 6, 1869). The following are the author's results:—*Tungstate of Copper.* In 100 parts:—Tungstic acid, 56·48; oxide of copper, 30·63; lime, 2·0; oxide of iron, 2·58; silica, 8·87; water driven off at red heat, 4·62. *On the Titaniferous Sand of the Chilian Sea-board, and on the Origin of that Sand.*—The analysis

of two varieties of this sand is quoted percentically, care being taken to analyse separately the magnetic and non-magnetic portions. The following quotation will give an idea of the composition of this material as met with at Punta Arenas (on the Straits of Magellan):—Non-magnetic portion—Titanic acid, 19·2; protoxide of iron, 29·7; peroxide of iron, 49·7; lime, 0·9; magnesia, 1·0. Magnetic portion—Titanic acid, 22·8; protoxide, 15·8; peroxide of iron, 61·5.

A Collection of Russian Minerals.—According to the *Scientific American*, the Russian Government has recently presented to the cabinet of the School of Mines, of Columbia College, U.S., a very choice collection of minerals. Conspicuous among them are nuggets of native gold, native platinum, iridosmine, large emeralds from the Urals (single and in clusters in the gangue), topaz, chrome garnet, malachites, &c.; in all 456 rare specimens.

Extracting and Separating Copper.—Mr. J. Elkington is the author of a paper in the *Revue hebdomadaire*, May 19, on the processes employed for this purpose. The principle applied by the author consists in employing electricity for dissolving the copper contained in the crude metal obtained by the usual smelting methods, and for depositing that metal galvanically upon plates of copper, causing the other foreign metals to fall to the bottom of the vessels in which the operations take place. Copper, containing very small quantities of silver, may be advantageously treated thus for the recovery of the last-named metal.

Two Cornish Minerals.—At the meeting of the Chemical Society on May 5 Professor Church communicated the analysis of two Cornish minerals. The one, restormelite, was obtained from the Restormel Iron Mines, and may be regarded as a variety of kaolinite, standing nearest to the lithomarge group. Its specific gravity and its hardness are nearly the same as those of lithomarge. In its percentage of silica and alumina (its chief constituents) it does not differ from that of lithomarge; but, while restormelite contains 7 per cent. of soda and potash, lithomarge contains a mere trace of these alkalies. Mr. Church considers restormelite as preserving in its alkalies more evident traces of its felspathic origin than we usually find in such alteration-products. The percentages obtained in six analyses were then stated.

MICROSCOPY.

Mr. Holmes's New Binocular.—Mr. Samuel Holmes has constructed a binocular by contriving to divide the object-glass along its whole length, and so splitting the pencil of light into two, which pass along two tubes to the two eyes. The following is the description of the instrument:—"My invention consists in the use of two object-glasses, or portions of two object-glasses, or of one object-glass divided into two parts, to supply through two eye-pieces a binocular and stereoscopic view of opaque or transparent microscopic objects while illuminated by reflected or transmitted light, and also in the use of certain mechanical means herein described, or their equivalents, for securing the motion in required directions, or rest in necessary positions of the optical parts of such combinations for obtaining monocular or

binocular vision. The objective—I take an ordinary object-piece, and by a circular saw divide it along its line of collimation, and afterwards rejoin the halves by screws and steady pins, until as an objective it is in as perfect a state of adjustment as before division. It is then capable of acting as an objective for one or two eyes, according to the position assumed by the two halves under the control of the mechanical part of the instrument when the direct light is stopped out. According to another method, I work the lenses of an achromatic object-piece out of divided and rejoined discs of glass, which when finished and fixed in a divided mounting temporarily held together for that purpose may be afterwards separated by dissolving out the cement by which the halves of the discs were originally conjoined. Or lastly, I make two whole object glasses, and fix one into each half of a divided mount, cutting away only such portion as will allow of proper approximation. This method is available for high powers and for binocular use only. In all cases I cut the usual screw-thread on the objectives to affix them to the body, and more surely secure their halves in their respective places in the divided body tube of the instrument by two small milled-headed screws."

Mr. Stephenson's new Erecting Binocular.—Of all the inventions which have been made since Mr. Wenham brought out his original prism for the binocular, that which has been described to the Royal Microscopical Society at its last meeting (June 8), by Mr. Stephenson, is the most remarkable. This new binocular has been used with powers as high as $\frac{1}{16}$-inch (immersion), with excellent results, and it promises to be an instrument superior even to the "Wenham." We must not anticipate the description of the instrument which is to appear in an early number of the *Monthly Microscopical Journal* [the microscope is now, we believe, being made by Messrs. Ross, the eminent opticians, of Wigmore Street]; but we may give a report of some of the remarks made by those who examined the instrument at the last meeting of the society:—Mr. Brooke said that from his observation of a fly's tongue exhibited under Mr. Stephenson's microscope, it appeared that the definition was not at all interfered with by the peculiar construction of the instrument. The two fields presented to the two eyes were equally illuminated by the two pencils of light diverging equally from the object, instead of being thrown off obliquely, as in Mr. Wenham's arrangement. The binocular effect produced also is more perfect than in the ordinary instrument. The two horny processes by which the fly's tongue is articulated and the series of tubes of which it consists were very well defined.—Mr. Slack said he had been struck with the perfection of the definition given by Mr. Stephenson's microscope, and the equality of the illumination in each tube when low powers were employed. With a power of $\frac{1}{3}$rd only part of the field was illuminated, but that portion afforded most admirable definition, and no noticeable errors were occasioned by the light impinging upon the edges of the prisms. It was certainly a very valuable invention.—Mr. Lee said he wished to correct the impression that Mr. Stephenson's microscope could be satisfactorily employed only with low powers. He had made examinations with Gundlach's $\frac{1}{16}$ immersion lens (about equal to a $\frac{1}{15}$th of the best English makers) which were quite satisfactory, the definition not being interfered with in any way. He believed that Mr. Stephenson had

brought before the society the most important improvement in the microscope since the days of Mr. Wenham.—Mr. Browning briefly explained his reasons for believing that the second prism used did not produce imperfect definition.—The President said that he would confirm Mr. Lee's observations concerning the use of high powers in Mr. Stephenson's microscope. The $\frac{1}{16}$th had been used with great advantage, and though both the fields of view were not equally illuminated under this high power, it would at once be seen from the construction of the instrument that only the art of the optician was required to make both tubes equally valuable in using high powers. Those who had had the pleasure of examining the instrument as well as himself would bear him out in the statement that the effect of giving an erect image is of the highest importance in cases where dissection was being conducted. The instrument devised by Mr. Stephenson contained no error of importance, and the slight want of flatness in the field could be readily obviated.

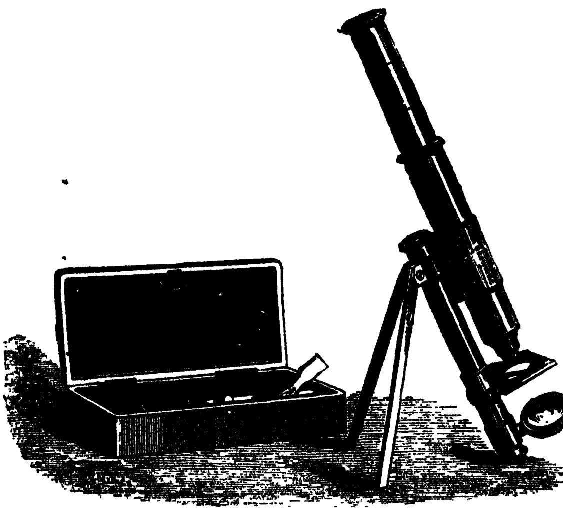
Hippuric Acid as a Microscopic Object.—In the *Journal of the Quekett Club* for April, Mr. T. Charters White gives an excellent account of the crystallization of this substance in order to prepare it for the microscope.

The Comparative Steadiness of the Ross and Jackson Plan of Microscope.—An important practical paper was lately read before the Royal Microscopical Society by Dr. Carpenter, giving the results of his observations on the relative steadiness of these two forms of microscope stands. Speaking of his experience of the two models in the last deep-sea expedition, he said: "When the ship was going under 'easy steam,' with either a fair wind or a light contrary breeze, there was enough *general* vibration to produce a considerable *differential* vibration in any microscope liable to it, and thus to occasion a decided tremor in the image even when only moderate powers were employed. But when we were steaming with full power against a head-sea, the general vibration became so great as to be the severest test of the mechanical arrangements of our microscopes. Now, it happened that whilst my own instrument—a portable binocular microscope weighing *less than seven pounds*, which is my usual travelling companion—is constructed on the Jackson model, Professor Wyville Thomson was provided with an instrument of about the same scale, but heavier by some pounds, made upon the Ross model; and we thus had an opportunity of fairly testing the two plans of construction under circumstances peculiarly critical. The difference in their performance was even more remarkable than I had anticipated. I found that I could use a $\frac{1}{4}$th-inch objective on my own microscope, with an even greater freedom from tremor in the image than I could use a $\frac{2}{3}$rds-inch objective on Professor Wyville Thomson's. In fact the image 'danced' very perceptibly in the latter, even when the $1\frac{1}{2}$-inch objective was in use."—*Monthly Microscopical Journal*, April.

An American Graduating Diaphragm.—Mr. Zentmayer has described an instrument of this kind in the *Journal of the Franklin Institute* (February. See also *Monthly Microscopical Journal*, June). It has been described also in the *Chemical News* by Mr. Henry Morton. It consists of two cylinders or rollers with parallel axes and surfaces in contact, having similar conical grooves on their surfaces, and fine teeth cut at one end of each, which, gearing together, cause them to rotate in unison. There is, theoretically, an objection to a dia-

phragm of this construction, from the fact that its opening will not always be in the same plane—that is, the smallest cross-section of the space between the rollers will not always be equidistant from a plane at right angles to the line of sight and passing through the axes of the rollers. With the larger opening, this smallest section will be nearest to, and with the smaller, farther from, such a plane. In practice, however, this difference is so small as to be entirely unimportant, and may even, in some cases, be turned to advantage. There are other forms of gradually adjustable stops which have been employed with more or less success, but few, according to Mr. Morton, involving so many elements of durability and convenience.

Browning's Pocket Microscope.—Mr. John Browning has recently turned out a pocket microscope, which we have much pleasure in commending to the notice of those of our readers contemplating a sea-side tour. Its



general features are shown in the figure. It has two objectives, 1-inch and 2-inch, and a large-field eye-piece. The workmanship, like that of all Mr. Browning's instruments, whether astronomical or general optical, is excellent. This instrument—the most portable yet contrived—is made with the body in two parts, one sliding into the other; the outer portion also slides down through the opening in the stage, which carries the objects. Two legs are hinged at about the centre of the instrument; the rod or tube, on which the reflecting mirror is fixed, forms a third leg. Thus, when the two hinged legs are open, the instrument has a firm tripod stand. These legs being opened, the lower part of the body drawn up through the stage, and the eye-drawer withdrawn from the body to about the same length is all that is required to set up the instrument ready for use. The tube spoken of as carrying the reflecting mirror and forming one of the legs has a fine screw on the inside, and a milled head at the top. This screw gives a fine adjustment. The instrument has an eye-

piece with a large field, and good objectives. The height, when set up for use, is about one foot, and the dimensions of the case which contains the instrument with two objectives, dipping tubes, and pliers complete, is only $6\frac{3}{4}$ inches long, 3 inches wide, and $1\frac{3}{4}$ inch long.

PHOTOGRAPHY.

Combination Printing.—The advantages of being able to effect a judicious combination print from several negatives are more and more impressing themselves upon the photographer. One of the most familiar examples of this kind of printing is the putting in of skies in prints otherwise quite cloudless. In the early days of the art, a white sky in a landscape was a *sine quâ non*; at the present time such a picture would not be tolerated. The landscape photographer keeps a set of negatives of clouds taken under different conditions of lighting, so that he can select for any landscape a fitting sky. This is the easiest kind of combination printing, because it does not require any exactness in registration; but far different is the case when a number of figures have to be printed with a landscape background from different negatives: here the greatest mechanical nicety is required to prevent the junction-line of the two from being noticed. To such a state of perfection has this department of photography now been carried, that it is not merely possible but quite easy to combine the parts of several negatives in one print, in such a manner as to produce a mechanically perfect whole—whether it be artistically perfect depends of course upon the harmony that exists between the several pieces of which the picture is made up. A landscape lighted from the right while its figures are lighted from the left furnishes an example of incongruity. Within the past few weeks, a patent has been obtained by Mr. B. J. Edwards for a printing frame, so constructed as to permit of combination printing being effected with the greatest possible efficiency; and as a result of what may be obtained by its means, it will be easy to have a photograph of one's own drawing-room taken (which requires a very long exposure in the camera) and introduce in it the members of the family, taken in the photographic studio under those circumstances of lighting most conducive to success in portraiture. This kind of printing is invaluable for picnic groups, cricketers, and other bodies, who may be singly taken in the studio, and yet form a completed group arranged in their field—a negative of which will have been separately taken.

Photographic Journalism.—The demise of a weekly journal, *The Illustrated Photographer*, is announced. The reputation it bore was latterly of an unfavourable kind, a good deal of personal and scurrilous matter having been introduced, without a corresponding counterpoise of ability. Its best illustrations, too, had occasionally done previous duty in the pages of other illustrated periodicals; and this, together with incompetency in the editorial management, could lead to no other result than that which has befallen it. There are now only two journals devoted to photography in this country—viz. *The British Journal of Photography*, which is in its seventeenth annual volume, and *The Photographic News*, in its fourteenth volume. *The Journal*

of the Photographic Society now contains only the transactions of that body, and is only published when a meeting of the Society is held, or eight times a year. Several of the foreign journals live, for the most part, on the English ones; the once original *Humphrey's Journal* (New York) is probably the most noted specimen of this class.

Permanence of Photographs in Asphaltum.—The durability of photographs obtained by the Pouncy process has been called in question. The process is based on the fact that bitumen of Judea, when dissolved in a suitable menstruum and applied to paper or any other surface, is rendered insoluble in proportion to its exposure to light. This property of asphaltum has been long known, but Mr. Pouncy mixes with it printers' ink; hence when the photograph is developed by turpentine the blacks are composed of printers' ink and bitumen. The question that arose is this—Seeing that bitumen is regarded by artists with very great aversion on account of its notorious bad qualities as a permanent pigment, will not its presence in the blacks of Pouncy's photographs be also objectionable? To this Mr. Pouncy replies, in effect, that the blacks or body of his pictures are not formed of bitumen alone, but of printers' ink mixed with it; that only the merest trace of bitumen is present in them, and that this portion is so altered by light as to have become incapable of "running" or cracking—the objections urged against bitumen when used as a pigment by the painters.

Negatives on Paper.—An effort is being made to reintroduce the taking of negatives on paper instead of on glass. A peripatetic photographer who desires to obtain—say—fifty views while on his travels will find an astounding difference in weight between fifty stout plates of glass, twelve or eighteen inches in dimensions, and the same number of leaves of paper the same size. Why paper should not be more used for negative purposes than it is we cannot say. The negative is taken upon, not in, the glass plate, and one would think that the same sensitive coating that is applied to the glass might also be applied to the paper. If this can be done successfully, which, from experiments now in course of being made and recorded, appears to be the case, then will a very great advance indeed have been made in photography as applied to the resources of the traveller.

New Salting Agent for Paper.—Some French photographers are recommending chloride of aluminium for salting paper instead of the chlorides of sodium and ammonium generally used at present. The chief advantage of using this salt appears to be that the image is retained well on the surface, and that consequently the details in the shadows of the print are better rendered.

Niepce de St. Victor.—Photographic science has received a sad loss by the death of M. Niepce de St. Victor, which occurred on April 7, after a few minutes' illness. He was seventy-two years of age. To him we owe very much indeed, including photography on glass plates, and printing upon albumenised paper. From 1847 to 1862 he presented to the Academy of Sciences in Paris no fewer than twenty-six *mémoires*, the subjects of which were chiefly photography on glass, photographic engraving, and heliochromy. His family being left unprovided for, photographers throughout Europe are making a subscription for them.

PHYSICS.

A Spectroscope for the Invisible Rays at the Red End of the Spectrum.—The Council of the Royal Society recently voted some funds to Mr. C. Brooke, F.R.S., for the construction of a spectroscope to search for lines in the invisible rays beyond the red end of the spectrum. The instrument is now in course of construction by Mr. Browning, F.R.A.S., and the prisms, which are made of rock-salt, have already been ground, polished, and tested. As glass stops many of the ultra-red rays, it is necessary to use lenses and prisms of rock-salt. When the apparatus is finished, a spectrum will be projected on a screen, and then search will be made for the lines by means of a delicate thermo-electrometer, with a thermo-pile having a fine slit in front. "It is found that the lines in the visible part of the spectrum contain no heat, so it is supposed that no rays of any kind fall at those points."

Physical Science at Cambridge.—After a series of long discussions and a considerable degree of opposition the new chair of Physical Science is about to be established at Cambridge. The Syndicate appointed for the purpose had to give up the notion of raising the funds by subscription from the several colleges. They confined their attention, therefore, to the means of raising sufficient funds only for carrying out the recommendations of the Physical Science Syndicate in their report dated Feb. 27, 1869. These were to provide the stipends of a Professor of Experimental Physics, of a demonstrator, and an attendant, requiring altogether a sum of 660*l.* per annum; also to provide a capital sum of 5,000*l.* for a new building, and 1,300*l.* for apparatus. The Syndicate are of opinion that these sums may be raised from the ordinary sources of revenue of the University, and that a small addition (viz. 2*s.* a head) to the amount of the annual capitation tax will suffice for the purpose.

Fall of a Large Meteorite.—At the meeting of the Geological Society on March 23, Mr. R. H. Scott, F.G.S., communicated an extract from a letter addressed to him by M. Coumbary, Director of the Imperial Observatory of Constantinople, containing an account received from M. L. Carabello of the reported fall of a large meteorite near Mourzouk, in the district of Fezzan, in lat. 26° N. and long. 12° E. of Paris. It fell on the evening of the 25th December last, in the form of a great globe of fire, measuring nearly a mètre in diameter; on touching the earth it threw off strong sparks with a noise like the report of a pistol, and exhaled a peculiar odour. It fell near a group of Arabs, who were so much frightened by it that they "immediately discharged their guns at this incomprehensible monster."

Meteorological Observations in the Captive Balloon.—The yearly Report of the Aeronautical Society contains an account by Mr. Glaisher, F.R.S., of the results obtained by him in his observations of the thermometer and barometer in the Captive Balloon. He says that the Captive Balloon supplied, in a most admirable manner, the power of repetition of observations within 1,000 ft., which no free balloon could do. Our knowledge of the humidity of the air, and of its temperature up to this elevation is very limited. Every ascent in the free balloon had proved that the theory of the decline of 1° of temperature in every increase of elevation was erroneous. In some ascents a decrease of more than 1° was met with in the first 100 ft., notwithstand-

ing the quickness of motion of the balloon on leaving the earth, and at such times there is no doubt that the real less reading at 100 ft. high was very much greater. On the contrary, in some of the ascents, a decline of 8° to 10° was met with within 1,000 ft. of the earth (*see ascents of July* 30, 1862; *August* 18, 1862, &c.), whilst in others but little or no difference was found within 1,000 ft. of the earth (*see June* 13, 1864); and again, in others at night, the air was of higher temperature on ascending. From all Mr. Glaisher's observations in the free balloon, it was found that the change of temperature near the earth varied greatly, and that it followed no constant law.

Mr. Joule in the French Academy.—At the meeting of the French Academy on the 30th ult., when Mr. Joule was elected to the place vacant by the death of M. Magnus as correspondent in the Section of Physics, the following was the return of the secret committee:—In the first line, Mr. Joule, of Manchester. In the second line and in alphabetical order, MM. Angström, of Upsala; Billet, of Dijon; Dove, of Berlin; Grove, of London; Henry, of Philadelphia; Jacobi, of St. Petersburg; Lloyd, of Dublin; Riess, of Berlin; Stokes, of Cambridge; W. Thomson, of Glasgow; Tyndall, of London; and Volpicelli, of Rome. Forty-three members took part in the election, with the following result:—Mr. Joule, 32 votes; Dr. Lloyd, 8; MM. Angström, Dove, and Volpicelli, one vote each.

Electric Currents caused by Contact of Metals and Distilled Water.—The *Comptes-rendus* for May 2 contains a paper by M. E. Becquerel on this point. The following are the results arrived at. The electrical effects produced by the contact of non-oxidisable metals and distilled water (chemically pure) are due, not to any special action of contact, but to the reaction of the water upon the gases condensed on the surface of these metals. The effects vary according to the molecular state of the metals and their temperature. As regards, however, the oxidisable metals, the electrical effects produced by heating them are due to the very slight layer of oxide adhering to their surface, whereby they are rendered positive towards the unpreserved metallic surface.

A New Salt-Water Galvanic Pile was described by M. Duchemin at the meeting of the Academy of Sciences of Paris on June 6.

The Leclanché Cell.—This cell, which is largely used in the batteries of the French telegraphic system, is thus reported on by Professor Morse:—"This battery consists of a prism of carbon for its positive pole, which is surrounded by a mixture of peroxide of manganese and carbon pulverised, filling the porous jar. This jar is put into the glass jar containing a solution of sal ammoniac; within the same glass jar and solution is a prism of amalgamated zinc, forming the negative pole. Its action is thus: On closing the circuit, the sal ammoniac is decomposed, the chlorine of the solution is absorbed by the zinc, the negative pole; while the hydrogen and the ammonia pass to the positive pole, reducing the peroxide of manganese. According to the inventor's explanation, 'the peroxide of manganese mixed with carbon being a good conductor of electricity, the system may be considered as a single fluid element, in which the positive pole is formed of an artificial metal having a great affinity for hydrogen.'"

Supra-Annual Cycles of Temperature in the Earth's Surface Crust.—In a paper laid before the Royal Society in April, Professor C. Piazzi Smyth,

F.R.S., discusses the completely reduced observations, from 1837 to 1869 inclusive, of the four great earth-thermometers sunk into the rock of the Calton Hill, at the Royal Observatory, Edinburgh, by the late Principal Forbes, pursuant to a vote by the British Association for the Advancement of Science. Leaving on one side the several natural-philosophy data which have been investigated from smaller portions of the same series of observations both by Principal Forbes and Sir William Thomson, the author applies himself solely to trace the existence of other cycles than the ordinary annual one in the rise and fall of the different thermometers. Of such cycles, and of more than one year's duration, he considers that he has discovered three; and of these the most marked has a period of 11·1 years, or practically the same as Schwabe's numbers for new groups of solar spots. Several numerical circumstances, however, which the author details, show that the sun-spots cannot be the actual cause of the observed waves of terrestrial temperature, and he suggests what may be; concluding with two examples of the practical use to which a knowledge of the temperature cycles as observed may at once be turned, no matter to what cosmical origin their existence may be owing.

Fluids in Crystals.—Spectrum analysis, says the *Chemical News*, has been applied by Vogelsang and Geissler to the difficult question of determining the chemical nature of the fluid found enclosed, in minute quantity, in the cavities of certain quartz-crystals. Fragments of quartz were placed in a small retort which was connected with an air-pump and exhausted; then, by the application of heat, the quartz decrepitated, and the evolved vapour was examined in a Geissler-tube. The presence of carbonic acid was thus abundantly proved, and this was confirmed by the turbidity which it produced in lime-water.

The late Professor Lamé and Professor Magnus.—During the past quarter we have lost two great luminaries of the natural-philosophy world. Professor Lamé was a member of the Institute since 1843. He was a very celebrated physicist and mathematician, was born in 1795, educated at the École Polytechnique, and was for some time engineer in the Russian service. On his return to France he was appointed Professor of Physics at the above-named school, and remained in that capacity until the year 1845, when he was elected Examiner of the school. In the year 1848 he was appointed Professor of the Faculty of Sciences at Paris. Among his very many published works, those on mathematics and on the elasticity of bodies are the most celebrated.—Dr. Heinrich Gustav Magnus, of Berlin, died there on the 4th inst. He was born at Berlin on the 2nd of May, 1802, and took the degree of Ph.D. at Berlin University in 1827, his inaugural dissertation written in Latin bearing the title "De Tellurio." In 1834 he was appointed Extraordinary Professor of Natural Philosophy at Berlin University, and in 1845 became Ordinary Professor for the same subject. He largely contributed to extend our knowledge of physical sciences, but, owing to the great mass of his various contributions, it is quite impossible to give here even a brief outline of his labours. Professor Magnus was a member of several scientific societies and institutions, and carried on a regular correspondence with the foremost scientific men in the civilised world.

A Lecture Experiment demonstrating the Expansion of Water is thus described by Herr F. Rüdorff. In order to exhibit the effect of the expansion of water when freezing, the author fills with distilled and previously well-boiled and cooled water, a cast-iron cylinder having the following dimensions:—Height, 160 mm.; diameter (external), 50 mm.; thickness of solid iron, 15 mm. After having been filled with water, this apparatus is closed by means of a plug screwed into the neck, and the cylinder is next placed in a mixture of three parts of snow or pounded ice, and one part of common salt; after about forty minutes the cylinder bursts with a loud report. It is essential for the success of this experiment that the plug fits very perfectly, and that the cylinder, after having been filled with water, be placed for some time in ice. The wooden pail which contains the cooling mixture should be rather roomy, and be covered with a stout towel to prevent the spirting about of the contents at the time of the bursting.—*Berichte der deutsch. Chem. Gesell.*, No. 2, 1870.

Cement for Glass Prisms.—We have received so many letters of inquiry on this subject that we publish the following extract from the *Scientific American* of June 4:—The best cement for putting together glass prisms, to be filled with carbon disulphide (bisulphide of carbon), is a mixture of common glue and West India molasses. Make a tolerably liquid glue, and add about one eighth the quantity of common molasses. Lay the metal or glass form, ground to a suitable angle, with its face up: then place the glass plate upon it, and apply the cement on the under side with a brush, and allow it to flow by capillary attraction between the glass and the form: repeat this operation several times if necessary. This is better than to wet the edge of the glass at the outset before attaching it to the prism. The stopper to the prism can be cemented in the same way, and in filling with the carbon disulphide, always leave a small space for the expansion of the liquid. The prisms want to be kept in a cool, dark place, and ought not to be agitated previous to using, as the light is in that case unequally refracted.

A Peculiar Telegraphic Improvement.—The (American) *Telegrapher* describes a valuable improvement in relay magnets, by W. W. Smith, of Cincinnati, Ohio. The improvement consists in arranging the connections of a relay so that the main circuit is divided, one half passing through each helix, and uniting again on the opposite side, instead of having the conducting wire of the two spools continuous, as in the usual manner. It will be seen upon a moment's reflection that, by changing the connections of a magnet of the usual form, and arranging them upon Mr. Smith's plan, the total resistance will be reduced one-fourth of the original amount, while the two helices will exert their magnetic influence in conjunction upon the soft iron cores, as usual.

ZOOLOGY AND COMPARATIVE ANATOMY.

A Peculiar Specimen of Hyalonema.—Of the specimens of Hyalonema in the Museum of the Academy of Natural Sciences of Philadelphia there is one, according to Professor Joseph Leidy, which appears to him to be

somewhat significant. The fascicle would appear to have been withdrawn from its sponge body and lain some time in the sea before it was found. This is inferred from the fact that the Polythoa crust reaches to within an inch and a half of the end, which in the natural condition is inserted in the sponge mass. Two shark's eggs are also attached to the fascicle by their tendrilled extremities, and one of the tendrils clasping the fascicle is included in the polyp crust.

Dredging in the Gulf Stream.—The *Bulletin of the Museum of Comparative Zoology*, No. 13, contains Professor Louis Agassiz's important Report, which besides giving his own results sums up those of other similar expeditions. According to Professor Agassiz, the fauna of the reef, consisting mainly of corals, extends to ten fathoms only. The second zone, "a muddy mass of dead and broken shells, broken corals, and coarse coral sand, is chiefly inhabited by worms, and such shells as by their nature seek soil of this character, with a few small species of living corals, some *Halcyonarians*, and a good many *Algæ*." This extends seaward "from a few miles" off Cape Florida to "twenty miles and more off Cape Sable." "A third region, or zone, beginning at a depth of about fifty or sixty fathoms, and extending to a depth of from two hundred to two hundred and fifty fathoms, constitutes a broad slanting table-land, beyond which the sea-bottom sinks abruptly into deeper waters. The floor of this zone is rocky; it is, in fact, a limestone conglomerate, a kind of lumachelle, composed entirely of the remains of organised beings, animals now living upon its surface." Algæ are but sparsely represented upon the plateau, and, though the animals are abundant, the species are generally of small size and belong to genera either identical or closely allied to those of the Cretaceous period. The deep sea proper beyond this zone lies upon "a uniform accumulation of thick adhesive mud, with a variety of worms and such shells as seek muddy bottoms." Professor Agassiz thinks that if the bottom in these depths was rocky, animal life would be "as varied and as numerous comparatively as are the alpine plants on the very limits of perpetual snow."

Mr. Barrett's New Stentor.—In the *Monthly Microscopical Journal* for April Mr. Barrett describes and figures this supposed new species. The animal lives attached by the lower portion of its body within a tubular case, which is firm, of a light brown colour when young, slightly transparent, becoming opaque and of a darker colour with age. The animal is trumpet-shaped when extended, ovoid when contracted. The body is covered with long hairs, standing erect from it, and continued round the expanded head, on a plane, external to the cilia, which are placed on the free edge of the velum, which runs round within the edge of the ear-shaped disc. Where the perpendicular portion is joined by the lower portion on the left side two ciliated processes are sent to the centre of the head, and between these the mouth, which is funnel-shaped, is placed, and Mr. Barrett thinks also the anal opening. The mouth is lined with vibrating cilia, and leads down to the stomach sacs through a short œsophagus. Close to the cloaca and near the mouth is placed what he takes to be the nervous ganglion. It is oval-shaped, sends down two branches the whole length of the body, one branch to the mouth and one round the ciliated head. The body is composed of a semi-transparent bluish-white granular sarcode, enclosing many vacuoles;

the contractile vesicles are placed one on either side of the largest stomach sac. The body has no division between it and a tail-like foot-stalk, and it has neither masticatory bulb nor teeth, both of which are present in tube-dwelling rotifers. The length of the case is $\frac{1}{50}$th, and of the extended animal $\frac{1}{25}$th of an inch.

The Management of the Fresh-water Aquarium.—Mr. Charles B. Brigham has a paper in the *American Naturalist* for March, describing the mode of managing the fresh-water aquarium. For the tank he says that a glass rod about a foot in length and a quarter of an inch in thickness will be of use in moving the specimens into place when disarranged. Too much cannot be said against unnecessarily meddling with the specimens in the aquarium; a slender rod with a sponge attached to the end of it will be useful in removing the confervæ from the sides of the tank; a small gauze net three or four inches in diameter is often needed to remove dead or objectionable specimens; an india-rubber pipe several feet in length affords the simplest method of drawing off the water of the tank; a fine gauze should be placed over that end of the pipe which is in the tank, otherwise the specimens may pass through it and be lost. Should the water in the tank become impure by any means it can often be purified by the following simple method; take a small earthen flower-pot holding about a pint, and insert a piece of sponge tightly in the opening at the base so that when the water is placed in it it will pass through the sponge only drop by drop; the pot being filled with one-third powdered charcoal and two-thirds water, place it over the tank and let it empty itself into the aquarium. The effect of this simple contrivance is astonishing, and it will often save one the trouble of arranging the aquarium anew.

The Habits of the Aye-Aye.—It is so generally supposed by naturalists that Professor Owen's notion of the evidence of design illustrated by the habits of the aye-aye is a mistaken one that the following letter, addressed by Mr. Humphry Sandwith to a weekly contemporary, is of especial interest:—"As a simple matter of fact, allow me to state that I kept a living aye-aye (now preserved in the British Museum) in a large cage in the Mauritius, and as its food I gave it the maggot that infested branches of a species of acacia. The animal used to spend its evenings in feeding, as follows. It listened attentively at the branches, tapping occasionally the most perforated parts; it then tore off pieces of the wood around the maggot-hole, inserting the peculiar long finger as a probe from time to time, and ended by extracting the maggot by means of this long finger and its strong rodent teeth. I have seen the operation scores of times."

Animal Life at Great Depths in the Ocean.—Those who wish to read a short but well-condensed account of the development of our knowledge of this subject will find it in a paper by M. A. J. Malmgren, in Siebold and Kölliker's *Zeitschrift*, April 1. Beginning with the earlier observations of Sars, Koren, Danielssen Loven, and others, it then deals with the labours of Edward Forbes, Dr. Wallich, Milne Edwards, and at last brings us down to the inquiries of Carpenter, Thomson, and Gwyn Jeffrys.

The Cornea of the Bee.—In a paper published in the April number of the *Journal of the Quekett Club* Mr. B. T. Lowne gives an excellent description of this structure. "The external layer is far thicker than in the fly, although

it presents no trace of structure. It follows the contour of the facets on the surface of the deeper layer. The deeper layer consists of numerous thin laminæ, each being composed of a vast number of parallel fibres. It further appears to consist of hexagonal prisms, vertical to the surfaces of the cornea, and to the laminæ of which it is composed. These prisms are not, however, separable, although the laminæ and the fibres of which they are composed are easily separated by treating the section for a short time with a solution of caustic potash. The surfaces of this deeper layer of the cornea may be seen to be covered with convex facets, a facet corresponding to either extremity of each vertical prism, the external facets being, however, best marked. In section this layer is seen to be marked by several hundred crenated lines, caused by the laminæ of which it is composed; the crenations follow the contour of the facets, the middle layers being almost or entirely without crenations. These lines, as well as the indications of the division of the cornea into hexagonal prisms, are best marked in the external portion of the layer. Numerous minute nuclei appear between the laminæ. When viewed by the aid of polarised light and a selenite, these modifications of structure all become beautifully apparent. Both layers of the cornea polarise, and the colour of the transmitted beam varies from red to green, according to the thickness of the section simultaneously in both."

How to Mount Spiders for Cabinets.—In M. Thorell's fine 4to on European Spiders, which singularly enough is published in Upsala, and yet printed in the English language, the following instructions are given:—"The spider is first killed, either by the vapour of ether or by heat, and is impaled by an insect-pin, which is passed through the right side of the cephalo-thorax; the abdomen is then cut off close to the cephalo-thorax, and the cut surface dried with blotting-paper. The head of another insect-pin is cut off, and the blunt extremity introduced through the incision into the abdomen, up to the spinners. The abdomen thus spitted is inserted into a large test-tube held over the flame of a candle, the preparation being constantly rotated till dry, avoiding the extremes of too much or too little heat—the firmness of the abdomen being tested every now and then with a fine needle, till it is so firm as not to yield to pressure; the front extremity of the pin is now cut off obliquely, and the point thus made inserted into the cephalo-thorax, the two halves of the body being thus again brought into apposition. The animal may then be mounted as usual."

New Tailless Batrachians.—At the meeting of the Zoological Society on June 9 Dr. A. Günther communicated an account of the species of tailless batrachians recently added to the collection of the British Museum, amongst which was a new diminutive frog, recently discovered by Dr. Cunningham in Fuegia, and proposed to be called *Nannophryne variegata.*

African Swallows.—At a meeting of the above-mentioned society on May 12 Mr. R. B. Sharpe read a paper containing a full account of the swallows (*Hirundinidæ*) of Africa, and pointed out their characters and geographical distribution. Particular attention was drawn to the affinities of the African *Hirundinidæ* with those of the New World, and also to the representation of various species by smaller races or sub-species throughout the Æthiopian region. Thirty-eight species of swallows were enumerated,

of which number thirty were stated to be peculiar to the continent of Africa, and two to Madagascar and the adjacent islands. Two species only were common to India and Africa, and the remaining four were migratory throughout the Palæarctic and Æthiopian regions.

The Prosectorship in the Zoological Society.—It is rumoured, and we believe with some foundation, that this post, vacant by the retirement of Dr. James Murie, is not likely to be filled up for some considerable time.

Dimorphism in the Higher Worms.—The *American Naturalist* for March gives an abstract of M. Claparède's observations on the Annelids, published in the *Bibliothèque universelle.* It states that in his account of the Annelids of the Gulf of Naples he confirms the discovery of Malmgren, that Heteronereis is a form of the old genus Nereis. He states that Ehlers, in 1867, in his *Die Borstenwürmer*, a work on the higher Annelids, has shown the undoubted specific unity of *Nereis cultrifera* and *Heteronereis lobulata*; of *Nereis pelagica*, and *Heteronereis grandifolia*; of *Nereis Dumerilii* and *Heteronereis fucicola*; of *Nereis vexillosa*, and *Heteronereis Middendorfii*; of *Nereis fucata* and *Heteronereis glaucopis*, and another Heteronereis form to *Nereis Agassizii* and *Nereis virens.* He thinks the Nereids are transformed into Heteronereids at the time of sexual maturity. Claparède states, however, that all the species of Nereis do not have a Heteronereid form, as the species of Nereis far exceed in number those of the so-called genus Heteronereis. He thus concludes: "The fact of animals presenting two sexual forms is not entirely new. The beautiful observations of MM. Leuckart and Mecznikow, and those of M. Schneider on the *Ascaris nigrovenosa*, have made us acquainted with analogous cases among the Nematodes, where one of the generations, it is true, is hermaphrodite, and the other presents separate sexes. But, among the Acalephs, certain Geryonidæ (*Carmarina*), according to M. Haeckel, and among the Nematodes, the *Leptodera appendiculata*, according to M. Claus, present two sexual forms, for each of which '*gonochorisme*' is the rule. The history of the Axolotls, which M. Duméril has acquainted us with, offers certain points of analogy with that of *Nereis Dumerilii.*"

Monstrosities among Infusoria.—Mr. J. G. Tatem has described and figured some very curious examples of the teratology of the Infusoria. Both the account and the figures will be found in the *Monthly Microscopical Journal* for April. The first figure shows a *Trachelias anas*, in which the lip, or brow (as it is sometimes called), is inordinately prolonged, somewhat spirally coiled, and clothed with longer and stouter cilia than usual. There is also shown a *Chilodon cucullulus*, where there is also a monstrous development of the same part, the lip projecting into an elongated proboscis-like appendage, which, as seen waving to and fro, and twisting with every movement of the animal, presented a singularly grotesque appearance. In contrast with the two preceding there is a charming *Vorticella.* Elegant and attractive as are the several species of *Vorticellæ*, it surpasses them all in beauty, and it is with reluctance only that the author could be brought to regard it as a monstrosity. Met with on several occasions and in widely distant localities, he may fairly question if it may not rather claim the importance of a named variety, under that of *Vorticella convallaria*, var. *monilata.*

Wanted Corals to Exchange.—The Museum of Comparative Zoology of Cambridge, United States, is prepared to furnish extensive collections of all the rocks and loose deposits found upon and about the keys and reefs of Florida, also complete collections of the corals, in fresh and well-preserved specimens, in exchange for recent and fossil corals from other parts of the world.

The Australian Mud-fish.—Mr. Gerard Krefft has communicated to the Zoological Society a description of a new and very remarkable animal, allied to Lepidosiren, recently discovered in the freshwaters of Queensland. Mr. Krefft considers this animal to be an amphibian, and referred it to the genus Ceratodus of Agassiz, proposing to call it *Ceratodus Forsteri*, after Mr. Wm. Forster, its discoverer. The figure of the creature shows that it resembles the Lepidosiren very closely.

Curious Malformations in Insects.—Mr. Henry Gillman, of Detroit, Michigan, U.S., records some interesting results of observations conducted in the neighbourhood of the south shore of Lake Superior.—*American Naturalist*, March. They refer especially to the dragon-fly. In an individual he specially observed, the skin had just been cast, and the wings, not having yet hardened, were quite soft and delicate to the touch. In one of the wings was a lump-like unexpanded portion, reducing the size of the limb nearly one-half. The malformation was similar in each of the instances noticed by him, and was so serious as to prevent the flight of the insect, it invariably falling to the ground on being thrown into the air, and being quite unable to raise itself. A like deformity, with like results, he had previously found to be not uncommon in the Ephemera, which is produced in such countless multitudes in the lake region. The only wonder is that creatures so fragile that almost the touch of a finger injures them should be brought into existence in such myriads, generally unharmed and perfect. He saw two examples of a more singular case of malformation in the beautiful pale green moon-moth (*Actias luna*). The wing was similarly dwarfed or contracted, a large portion towards the extremity being unexpanded and hardened. The colouring matter and fluids which should have passed down to perfect the development remained above in greenish blisters, protruding the skin of the wing on each side. On breaking this the contents escaped. By pressing those blisters it was possible to project the coloured fluid in any direction within the wing, the motions being quite perceptible in the increased brilliancy of colour of the parts where the fluid passed.

Rotation of the Embryos of the Frog.—In *Pflüger's Archiv* (1870, Heft 2 and 3) Herr Schenk describes the movements of the embryo in the ovum of the frog. The rotation which takes place from right to left requires from five to twelve minutes for its accomplishment. He states that it is entirely due to ciliary action—(1) because ciliary cells may be seen with the microscope, (2) because heat increases the rapidity of rotation, and (3) because acids arrest the movement.

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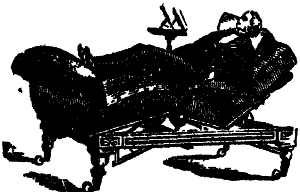
NO. 1 shows J. ALDERMAN's Patent Graduating Elastic Spinal and General Invalid Couch, which is fitted up with two, three, or four distinct graduating actions, so that an invalid can be adjusted imperceptibly to any position without being touched by the nurse, and free from all pressure, that being avoided by his Elastic Ventilating Mattress and his Patent Elastic Adjusting Foundation, so that a patient cannot become bed-sore by long confinement. They are also fitted with a Closet arrangement if required.

SAME AS SUPPLIED TO GENERAL GARIBALDI.



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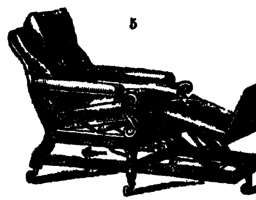

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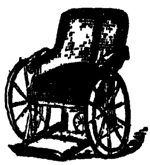
The following gratifying Letter has been received by MR. ALDERMAN *from* GENERAL GARIBALDI.

'Caprera, August 20th, 1868.

'Mr. ALDERMAN,—Some time ago I addressed to you a few words of thanks for your magnificent bed, which has been so great a comfort to me during my illness. Now I hear with regret that my letter has miscarried, and I willingly come forward again to express my gratitude to you. You have alleviated half my sufferings. My affectionate thankfulness will accompany you eternally. Accept a grasp of the hand from yours ever,

(Copy) 'G. GARIBALDI.'

No. 5, J. A.'s Patent Graduating Elastic Self-Adjusting Invalid Chair. This Chair, like the Couch, is made to follow nature in every respect, as the back, the arms, the seat, and the leg-rest are made to work at the same time, so that not a muscle of the patient need be disturbed. The arms are also made to put on and off in an instant, so that the patient may get on and off from either side while the leg-rest is out.



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No. 6 is J. A.'s Improved Self-propelling Chair, which renders an invalid perfectly independent, being able to run from room to room without any assistance.

No. 8 is J. A.'s Patent Portable Equilibrium Carrying Chair, which enables an invalid, however weighty, to be carried up and down stairs with perfect ease and safety; the Chair always adjusting itself, enables the two persons who carry to walk up and down stairs in the usual way, quite erect, with their arms straight down, which avoids any strain upon their muscles. It is also a perfect Easy Chair for the patient to sit in during the day, the carrying handles being made to hook on and off in an instant.

8

11

No. 11 is J. A.'s improved Three-Wheel Albert Chair, with a hood. It is elegant and easy.

THE GEOLOGY OF THE MONT CENIS TUNNEL.

BY PROFESSOR D. T. ANSTED, M.A., F.R.S., FOR. SEC. GEOL. SOC.

WITH A GEOLOGICAL MAP AND SECTION.

[PLATE LXIV.]

THE approaching completion of the great tunnel through the Alps, the most interesting and remarkable engineering work—certainly the most daring and speculative work—of any that has yet been undertaken for railway purposes, renders an account of its progress and of the condition of the rocks pierced especially interesting. At the present time, more than half the total distance has been completed from the Italian and nearly half on the French, and the rock at both ends being now the same, the structure of the whole mass of mountain penetrated may be estimated with something approaching to certainty. A recent visit by the writer of this article has induced him to believe that an account of the geology of the tunnel rocks would be found to be of general interest. His object will be not so much to describe the numerous and ingenious inventions and mechanical appliances adopted for perforating the rock and ensuring ventilation for the great distance of nearly four miles from each end, as to point out the condition and nature of the rocks pierced in comparison with those seen on the surface. We thus get an insight into the real structure of the Alps at this point, illustrated by a perfect section between seven and eight miles long, obtained by the aid of a tunnel through highly metamorphic rock, the central part being more than 5,000 feet perpendicularly below the actual surface of the ground. Besides the geological structure, several important physical problems will be found to be involved and will receive some explanation.

The part of the Alpine mountain system through which this tunnel is being pierced, is the watershed that separates the waters of the Dora, one of the principal tributaries of the Upper Po, entering the Po at Turin, and those of the Arc, a

tributary of the Isère and ultimately of the Rhone. Crossing the Rhone at Culoz, the railway is carried along the expanded valley forming the lake of Bourget to Chambéry, and thence, in the same direction, without any works of magnitude, but through scenery of very great beauty, till it enters the main valley of the Isère. Proceeding up that valley for a short distance and crossing the river, it turns aside to enter the valley of the Arc, passing Aiguebelle and traversing a narrow strip of granite rock till it reaches St. Jean de Maurienne, a small town in Savoy, celebrated for its numerous and frequent earthquakes, and situated at the extremity of a projection of this granite rock to the east, penetrating the altered jurassic rocks which flank the granite both on the east and west. The rocks here consist of a large series of gypsums and clay, including deposits of iron ore and some veins of galena. A little beyond St. Jean a calcareous rock is crossed, believed to be the representative of the nummulitic series largely developed in southern Europe, and of an age recognised as lowest tertiary. After crossing a long strip of this limestone the jurassic rocks reappear, and beyond St. Michel they are found to be altered and greatly metamorphosed, putting on an aspect so generally characteristic of old rocks, that the geologist not well accustomed to the Alps and to Alpine geology would be easily deceived by them. A very extensive series of these metamorphosed oolites forms almost the whole mass of the two western divisions of the Alpine system, called the Cottian and the Graian Alps, separated by the valley of the Dora Riparia, and except where interfered with by some patches of serpentine and euphotide, they are remarkably connected and uniform throughout. The tract of country separating France from Italy in this part of Europe affords one of the lowest and most accessible of the numerous passes across the crest or culminating axis of the Alps. This occurs near the Mont Cénis, and over the col or pass known by that name is constructed one of the many magnificent roads for which Europe is indebted to the first Napoleon, who, imitating Hannibal in ancient times, had previously brought his army from France to Italy.

The Mont Cénis pass, though long a difficult and troublesome mountain path, seems to have been known and used from time immemorial. It has only within the present century become a great highway connecting north-western Europe with Italy, and is on the whole well adapted for that purpose, its highest point being only 6,890 feet above the sea. It may be crossed, with comparatively few exceptions, during the whole winter, and it is not so subject to destructive avalanches as many of the passes. The general width between the two principal valleys of the Arc and the Dora is here only twelve English miles, but the

length of road over the pass corresponding to this direct distance is nearly twenty-three miles. The valleys on each side are always fit for travelling, except immediately after serious floods, when the torrents coming down from the narrow gorges, chiefly on the French side, sometimes tear up and destroy the road. The Mont Cénis pass is, however, on the whole very approachable on both sides, and offers no particular or exceptional difficulties. The railway now in operation and intended ultimately to pass through the tunnel and connect with the line already open from Susa to Turin, terminates at St. Michel, a small and miserable village, beyond which to Modana, the entrance of the tunnel, is a distance of upwards of ten miles. From St. Michel to Susa the mountain rail known as the "Fell line" completes the communication, and is an admirable substitute for a railway of the ordinary kind; but it must be entirely superseded when the tunnel is once opened.

Geologically, the road beyond St. Michel towards Lanslebourg, the last village before the ascent of the mountain, crosses an important and extensive portion of the metamorphosed jurassic rocks of Switzerland, including—(1) a series composed of quartzy conglomerate, quartzite, fine grained gritstone, and some argillaceous and calcareous schistose rocks with anthracite, corresponding as a series, but not in detail, with the middle member of the English oolitic series. These rocks are developed to a great thickness in the Graian Alps, and are identified in places by fossils; but they are very much changed and the stratification is rarely observable. Below this occurs (2) another large series of rocks, among which gypsum is very frequent and remarkable. The gypsum is associated with clays containing here and there organic remains of animals, and the whole appears to represent the lower oolite of England, including the great and inferior oolites and the liassic sands. Underneath this series we have (3) a very remarkable and persistent group of schists, steaschists, talcoseschists, and other varieties of schist, with numerous alternations of quartz and occasional masses of quartzite. In the small fissures and cavities of this rock are crystals of calcite, sometimes very beautiful and perfect. Much of the schist is very hard and compact, of a dark grey or blackish colour streaked by numerous quartzy threads, but the variety of appearance is very great. On the surface many of the schists weather into a paler grey and very fissile rock rapidly decomposing into a powdery mass, resembling at a distance a heap of loose sand. The kind of talus thus formed contrasts singularly by its smoothness with the abrupt and needle-shaped appearance of that part of the rock which has not yet been weathered, and that rises very grandly into mountain peaks.

The rocks thus described may be seen on the valley side

immediately on leaving St. Michel, where the mountain railroad comes into operation, and where the carriages of the French railway are exchanged for the very uncomfortable omnibuses of the American or Fell line. The map and section in the annexed Plate will show the order of the rocks and the way in which they succeed each other. The direction of the valley is at first nearly east, it turns after a certain distance to the south-east, and then towards the north-east, which direction it retains as far as Lanslebourg. The rocks have a general north-westerly dip, averaging about 50° in the tract of country between the Arc and Dora valleys which is the geographical axis of the Alps in this part. The line of railroad rises pretty steadily after leaving the valley of the Rhone at Culoz, at first slightly, afterwards, when the valley of the Isère is reached, more rapidly, the level of the valley bottom near St. Michel being nearly 3,500 feet above the sea.

The project of a railway tunnel to be cut through the crest of the Alps for the purpose of affording permanent and rapid intercourse between France and Italy at all seasons and without change of rail, was due to an Italian engineer, and has been carried on throughout under Italian superintendence. The first suggestion of such a tunnel is said to be due to Signor Medail of Bardonneche, who put it forward in a pamphlet published at Lyons in 1841. This pamphlet coming under the notice of the king (Charles Albert), the Minister of the Interior was ordered to make further investigations. The enquiry was referred to Professor Sismonda and a Belgian engineer (the Cav. Maus), who was at that time in Turin constructing the Turin and Genoa railway. After careful consideration it was agreed that the scheme was practicable, and that the position suggested was the best in that part of the Alps; but it was estimated that the tunnel could not be constructed in less than thirty-five years by the ordinary means then available, even making no allowance for drawbacks and accidents. M. Maus, noticing the large quantity of water-power on both flanks of the mountain, conceived the possibility of utilising this, and invented machinery adapted for the purpose; but though a model was prepared the machine was not constructed. At a later period the present perforating machine was perfected under the superintendence of the engineers, Messrs. Grattoni, Grandis, and Sommeiller. It is quite distinct from that proposed by M. Maus, and being worked by compressed air (compressed by water-power to a pressure of between six and seven atmospheres), the air is available for ventilation. The exact site selected was so near the ordinary line of road crossing the Mont Cénis, that the tunnel has always been called by that name, but it really cuts through the Alps under the

Mont Fréjus at a distance of nearly sixteen English miles west of the former road. Starting from Modana on the Savoy side, about ten miles higher up the valley than St. Michel (the present terminus of the main line), advantage is taken of the widening of the valley, and the railway is conducted by a zigzag up the side of the mountain into a small ravine to a height of 3,946 feet above the sea, being a rise of 346 feet from St. Michel, showing an average rise of thirty-five feet in a mile, or one in about 150. From this point a direct line through the mountain, drawn at right angles to the axis of the chain at that part, carries us under the Mont Fréjus to an open part of the valley of Bardonneche, an important tributary of the Dora, emerging at a distance of 12,220 mètres (13,365 yards), or about 7½ English miles. The direction of the tunnel is about NNW.-SSE. The railroad, after emerging from the tunnel, runs down the Bardonneche valley for another seven miles into the comparatively wide and open valley of the Dora, the ground being afterwards not particularly difficult for a railway, and much less liable than the upper part of the Arc valley to injury from floods and torrents. The road follows the course of the Dora without further divergence to Turin.

It will be evident to the reader, from this account of its position, that the tunnel has been driven through the crest of the Alps, across what would at one time have been regarded as being almost inevitably an axis of elevation, and it would have been presumed that in the course of the work, the primitive granite, considered not long ago to form the nucleus of all great mountain chains, must be crossed under circumstances involving perhaps extreme mechanical difficulties, large and incessant incursions of water, hard, tough, and contorted rocks, and all those indications of convulsion and disturbance inseparable from such conditions.

Fortunately the engineers who first projected this great work had not sufficient faith in the geological theories common then among the multitude, and not unsupported by very high authorities, to be frightened by the prospect of these probabilities. Perhaps they had not made acquaintance with them, and regarded the question as one of ordinary tunnelling through ordinary rock. At any rate they induced the Government of Sardinia (at that time possessed of both sides of the Alps) to listen seriously to their suggestions, and support the work, which was commenced about twelve years ago under auspices favourable in some degree, but with a prospect of a very long, if not endless, work. According to the methods at first adopted, the excavation of the tunnel, although commenced at both ends together, could only proceed very slowly at first, and was likely to be delayed when the great distance from the entry

began to be felt. Fortunately for the advance of the work, a method of boring rock, by machinery worked by compressed air conveyed in tubes to great distances without losing its elastic force, was introduced, and after certain modifications was found to work admirably. The compressed air is introduced to work a piston rod much in the manner in which steam is made use of in the small engines called donkey-engines, and the power thus obtained communicates a rapid stroke to a heavy boring chisel placed horizontally, followed by a recoil and slight twist. In the course of the first two minutes of an experiment made in the presence of the writer, a hole was bored about an inch and a half in depth, and of about the same diameter, in a large block of the very hardest and toughest quartzite. Seventeen such machines at a time can conveniently be placed and worked in the end of the tunnel, and the rock thus bored is afterwards blasted. The rapidity of the work is so great, that during a single month (May 1867) as much as 90·60 mètres (99 yards) has been completed on the Italian side only, being an average of about ten feet per day. The present rate of advance averages about eight feet per day on each side, and at this rate the work will probably be completed, and, if so, communication will be opened from one side to the other at the end of the present year.* The whole line is expected to be open for traffic in the course of the year 1871.

It has been stated above, that in order to secure a convenient and even a practicable railway line it was necessary to tunnel through the crest of the Alps, and that a part of the chain was selected where the crest was by no means the lowest. It may be thought that an easier line might have been selected, and to understand the advantage of the Mont Cénis line some consideration must be given to the physical geography of this part of Europe. The Alpine system, although a part of the great mountain axis of the old world, terminates for a short space with a broad shoulder of mountain land, forming the Maritime, the Dauphiny, and part of the Savoy Alps and reaching the Mediterranean. From the Lake of Geneva to the Mediterranean there is, therefore, a continued and almost uninterrupted succession of mountains. A part of this is a confused and most impracticable—and therefore little visited

* On July 15, 1870, a few days after which the writer was on the ground, the exact state of the tunnel was as follows:—

		mètres.		yards.
From the Italian side	.	6,643·20	=	7,265·45
From the French side	.	4,761·80	=	5,208
Remains to be done	.	815·00	=	891·34
Total length of tunnel		12,020·00	=	13,364·79

—country, rising in Mont Pelvoux to the height of more than 13,000 feet, though usually much lower. This district is more remarkable for the narrowness and ruggedness of its numerous valleys than for its lofty peaks, although these include Monte Viso and Mont Genèvre as well as Mont Pelvoux. It has also some passes, but they are inconvenient. The valleys are not only inconvenient and inaccessible, but offer no advantages to draw away the traffic from the direct line in which it has long been carried. For the purposes of a railroad reasonably accessible, valleys and narrow crests are far more important than low mountains; and so far as a tunnel is concerned, it matters little whether it is carried under a pass or under a mountain top. Thus it is that the valleys of the Isère and its tributaries on the Savoy side, and that of the Dora on the Italian side, are those which the tunnel can most usefully join.

The geological interest of a tunnel thus driven through a mountain axis—the tunnel being not only by very far the longest but the deepest below the surface that has ever been attempted—is very great and very varied. We are enabled by it, first, to compare the rocks as they exist within the earth under great pressure with the same rocks cropping out on the surface. Secondly, we are in a position to judge of the condition of rocks in the interior of the earth with regard to small slips, slides, faults, cavities, and various phenomena of contortion, as well as slight disturbance on a very large scale, and to an extent elsewhere unknown. Thirdly, we learn the condition of the interior with regard to water. Fourthly, we obtain direct knowledge of the temperature of the interior of the earth under circumstances of great interest.

All these matters are now determined in the Mont Cénis tunnel so far as they are likely to be determined, for the works are carried on nearly half a mile beyond midway from the Italian side; they are already under the culminating point of the crest, and the rocks now being removed from the French side are precisely identical with those long excavated from the other end, proving that there is nothing more to be learnt in this respect.

The rocks penetrated in the Mont Cénis tunnel consist, as we have seen, of highly metamorphosed material, and no fossils have yet been discovered in them; nothing organic, indeed, has been in any way indicated beyond the existence of anthracite, of which there are beds of some thickness and importance. All the rocks are described as belonging to the jurassic series, but under that name must be included the lias, and possibly some of the rocks of the recently established Rhætic series below. Similar beds of the same series contain fossils in other parts of Savoy not very distant, and of the general fact that the

whole group consists of metamorphosed secondary rocks there is no doubt and no difference of opinion among Italian and French geologists. In a memoir recently published by M. Sismonda,* one of the most able of the Italian geologists, this gentleman describes the delight of himself and his companion, M. Élie de Beaumont, when, in 1847, on their way from St. Michel by the Perron des Encombres towards Moutiers, they arrived at fossiliferous rock among this wide waste of metamorphosed schists, and found it exceedingly productive. He says: "We left St. Michel with the intention of reaching Moutiers before evening. At the Col des Encombres we found ourselves suddenly enveloped in cloud, which soon resolved itself into mist, and then into downright rain. We reached the great fossiliferous mass soaked to the very bones, and our servant and guide were no better off. Notwithstanding this, we spent another three hours in looking for fossils. When we had got all we could, we proceeded on our way, and did not reach Moutiers till eleven at night, in a condition that may be more easily imagined than described."

The fossils thus obtained included sixty-three species, of which thirty-seven only were determinable. Of these five were lower lias, fourteen middle lias, twelve upper lias species, the remainder being species found throughout the lias. The position of these rocks, as representing the lias generally, is thus placed beyond doubt; but it must not be supposed that this was the first discovery of these fossiliferous deposits in the anthracite group of Savoy. So long ago as in 1828 M. de Beaumont had alluded to it in his *Notice sur un gisement de végétaux fossiles et de bélemnites, situé à Petit-Cœur, près Moutiers, en Tarentaise*, and the fact is one long recognised in the geology of the Alps. The series here dips considerably to the SE., and rises again in the valley of the Dora, where it exhibits westerly and south-westerly inclination also at a considerable angle. There is a synclinal axis between the granite rock crossing at Aiguebelle and the gneissic rock to the south-east of the valley of the Dora.

In the earlier paragraphs of this article the general sequence of the rocks in the valley of the Arc has been mentioned, and it is easy to show that from the direction of the strike of the strata and the curvature of the valley before reaching Modana, those are again repeated in the tunnel. They may also be observed in crossing the Col de la Roux, which is a pass from Modana to Bardonneche, passing near the Mont Fréjus at an elevation of about 7,750 feet. As the level of the tunnel in the middle is about 4,213 feet above the sea, the rocks

* Nuove Osservazioni Geologiche sulle Rocce Antracitifere delle Alpi, del Com. Angelo Sismonda, Professore di Mineralogia nella Regia Università di Torino. Turin, 1867.

seen at the surface as they crop out, may be compared with the same rocks under a cover and pressure of about 3,500 feet of similar rock. By proceeding a few miles to the east, and mounting the Mont Fréjus to the observatory erected for the use of the tunnel, a height of 9,676 feet above the sea is attained, being about 5,463 feet above the tunnel below. The middle point of the tunnel is thus nearly 20,000 feet from the earth's surface horizontally, and between 5,000 and 6,000 feet vertically below; and we are enabled with facility to compare rocks of the same series altered by metamorphosis in the same manner, but in which the effect of exposure to diminished pressure and increased weathering is so strongly marked.

It may be sufficient to describe the tunnel rocks as follows: —Entering on the north side from Modana, after penetrating for a short distance the talus of fallen and weathered rocks from the mountain side, the cutting is carried through more than 2,000 mètres, or nearly a mile and a quarter, of coarse grits, fine grits, quartzites, quartz conglomerates, schists of various kinds, chiefly of a pale-grey colour, some calcareous rocks, and bands of some thickness of imperfect anthracite. Crystals of blende and of galena have been found in grits, but no veins or strings of ore. Crystals of dolomite were found occasionally. The principal bands of anthracite were about 1,200 yards from the entrance, and very marked bands of quartzite were met with at about 2,500 yards. Many of the rocks in this part of the work were tough, and required much time and patience to bore through and blast. They were much harder than those at the other end, and rendered the progress much slower. The rocks of this group were found to be a good deal contorted, and, though on the whole dipping towards the northern valley at a very high angle, they appear to have shown marks of great squeezing, affecting the different rocks very differently. No important faults were observed.

The rocks of the last-named group terminate with quartzite, and were succeeded by a large series of limestones and gypsums. Proceeding up the valley to cross the mountain crest to Bardonneche, the rocks that crop out successively do not give a very clear indication of the series met with inside the tunnel until we reach the quartzite, which is precisely identical. The schists and psammites are altered in colour, texture, and hardness, but they remain evidently of the same nature. The slight disturbances, and especially the marks of extreme squeezing, are not observable in the surface rock.

The gypsum deposit next succeeding continues for about 1,650 yards, and consists of a series of limestones and clays, containing gypsum in very large quantities in an amorphous state. On the surface these gypsums are presented in large

masses buried in loose clay. Within, the rocks are much more compact.

Next beyond the schists comes in a series of bluish and blackish-grey schistose rocks, alternating with quartz, and this extends not only to the end of the tunnel on the Italian side, but far beyond for a distance of several miles, as far as the Dora valley. Everywhere these schists present the same general character, but they vary infinitely in matters of detail—such as hardness, colour, and texture. They are traversed by strings of steaschist and quartz, and contain occasionally crystals of calcite, iron pyrites, and (though rarely) copper pyrites. Within the cutting there has been no discovery of any vein containing ore. Externally there are few special indications. Owing to the mixture of decomposing schist and hard, compact quartz, and the fact that the schist falls readily into shale, and from that into loose, powdery fragments, the mountains above where these rocks crop to the surface are singularly jagged and picturesque, and weather at an extremely rapid rate. At the surface they present little appearance of peculiar structure. Within the tunnel they are, as has been said, extremely interesting and varied, although the variations are strictly confined to matters of detail, and may be taken as the results of metamorphic action combined with enormous squeezing on stratified rocks, consisting of clay, sand, and some (though a comparatively small proportion) carbonate of lime.

There seems to have been everywhere observed some trace of stratification, showing itself on a small scale by imperfect lamination, and on a large scale by the position of bands of quartz. The sand of the original rock would seem to have been partly converted into crystalline quartz, partly left in grains to form very compact gritstone. The various compounds of the clay have become converted into a greenish talcose rock, with very smooth, irregular faces, splitting irregularly, with many intervening cavities of little breadth, filled, or partly filled, with crystalline carbonate of lime, often developed into beautiful crystals of various forms. Occasionally, but very rarely, these interstices were occupied by iron pyrites. Throughout the long distance (upwards of four miles) tunnelled through this rock no essential change seems to have taken place. It was easy to penetrate and not unsound, leaving ample time to brick in before any fall of the roof could take place. It was always dry, except when, on a very few occasions, small apertures were pierced containing water under pressure; but even then the quantity of water was not sufficient to cause trouble. The rock was close-grained and moderately tight, and blasts well in large blocks. Among the pieces removed by blasting are many measuring about a cubic yard,

and some much larger. The beds, being inclined at a high angle, are easily removed in large masses.

All geologists familiar with the appearance of rocks in mines and tunnels are aware of the variety of dip that frequently shows itself in the interior of the earth, and of the fact that the dip of the rocks at the surface is by no means always coincident with that of the interior. The difference may be small, but it is generally clear and well marked. It may often be observed even in quarries and deep railway cuttings. In the Mont Cénis tunnel perhaps the most striking fact seems to be that of a constant variety of dip, but always within certain limits. This is no doubt due in a very great degree, if not entirely, to the contortion consequent on squeezing; and this again is the result of the elevatory forces that have brought up these materials, originally deposited horizontally on a sea bottom, and now the crests of great mountain chains many thousand feet above the sea. With the variety of dip is observed at the same time a singular complication of small slips and troubles. None of these seems to have been important or due to any other cause than the same contortions so prominently illustrated. Still the phenomena of glazed surfaces, slickensides, and smoothed, polished faces in close contact are to be observed almost incessantly, and they are accompanied by a few, but very few, open cracks and fissures, generally consisting of small pockets partly filled or lined with crystals of quartz, calcite, or dolomite, and crystals of iron and copper pyrites. Galena and blende are also found but are very rarely distributed, and in exceedingly small quantities, in the same localities.

So far, then, as the general appearance and construction of the rocks can be determined by aid of the extensive and remarkable excavation now in progress and nearly completed, we may say that they confirm what has been already observed in mines and tunnels of smaller extent in similar rock, and show the phenomena of metamorphosis of secondary rocks (originally, no doubt, fossiliferous and regularly bedded) to present the same general character in the interior of the earth that they do nearer the surface. No intimation whatever of the existence of older or harder or more altered rock is seen. The rocks are not, perhaps, quite so hard or tough in the middle of the tunnel as they were found to be near the entrance, but they retain all their characteristics throughout, and we may fairly presume that the mass of the metamorphosed jurassic rock not yet converted into granite or protogine retains now, both in the interior and near the surface, the same condition that was induced when the metamorphism was first effected, and the elevation of the mountain mass of the Alps was completed. All the phenomena of slides and small faults conform accurately

with what has been already observed in other places, and with other rocks that have been exposed to similar action; and thus the geologist is strengthened in his own conclusions, and satisfied with the correctness of the conclusions drawn by others in this department of geology, by the additional light thrown on his pursuits by the study of the works carried on in this remarkable tunnel.

While alluding to this part of the subject, it would not be fair to exclude mention of a supposed discovery of a rounded pebble, said to have been found in a narrow dry cleft or fissure, from four to six inches in width, near the middle of the tunnel. The cleft was very irregular, and was partly filled with crystalline quartz, and was said to be open above. The pebble was oval, and somewhat resembled the undular concretions of limestone or ironstone often met with in slags and shales. The enclosing rock was a talcose schist, with quartz of the ordinary kind. As there was a possibility that the specimen might have belonged to the rock, it was carefully broken, in the presence and at the request of the writer; but the structure showed it to be gritstone rounded by water. The specimen had been brought to the resident engineer by the foreman of the works, who had not before or since found any curiosities, and who claimed to have taken it himself from underground. The story is given as it was related.

The condition of the interior of the earth with regard to water is a subject on which it may be expected that great light will be thrown by the perforation of the Alps. A clean cut through seven miles of rock under a mass of 5,000 feet of overlying material nearly of the same kind may be expected to afford information of great value in this respect. The anticipations that might have been made, judging from experience in other tunnels, are, however, not altogether borne out by the reality, and the result is almost negative. The total quantity of water entering the tunnel has at no time been large, and, compared with its great length, must be regarded as wonderfully small. The average quantity is stated not to exceed one litre per second from each end, or less than 40,000 gallons per day from the whole excavation as at present completed. This quantity has been increased from time to time, when certain open but very narrow fissures and small cavities have been reached. In these cases there has generally been a rush of water, evidently under pressure, but the total content of the water cavities, including all their communications, has at no time been very large, and they have each in succession been exhausted, a very few days being generally sufficient for this purpose. In the case of the largest of them a quarter of a million gallons of water would seem to represent the full con-

tent. As this corresponds to about 40,000 cubic feet of space, some idea may be formed of the nature of the largest of the fissures; and as the water was very quickly drained off, it is evident that no open communication could exist with any other fissure, or with the surface. There has been no seasonal change in the quantity of water entering at either end. The pressure of the water in the cavities reached was sufficient to throw a jet forwards into the tunnel to a distance of as much as 20 feet, this lasting in one or two cases for some hours. No exact record seems to have been made of these fissures. When examined, the largest of them was not more than from four to six inches, and they do not appear to have extended with any regularity even across the tunnel. The largest of these water-containing fissures was reached in 1861, not very long after the commencement of active perforation. Another, of some importance, was cut in 1867, when the middle had nearly been reached. Both of these were on the Italian side. Others have been cut on the French side, differing little in the circumstances, but generally smaller. The contact of rocks of very different nature does not seem to have been marked by the presence of springs, nor did the limestones on the French side, though of considerable extent, yield more water than the compact schists on the Italian side. The temperature of the water varied considerably. That of the largest spring, reached in 1861, was 64°; that of the next largest, reached in 1867, was 79°. The water in the clefts on the French side was between 60° and 62°. The waters, on being roughly analysed, were found to contain oxide of iron, sulphate of lime, and sulphate of magnesia. As crystals of dolomite have been found, and there are large deposits of gypsum, no surprise can be felt at this result. It is important, however, to notice, that even at the distance of midway in the tunnel, and with so large a depth of rock above, the waters are not in any sense thermal, and do not proceed from deep sources. The result is that which would take place of necessity, if water percolating from above should find an outlet in the interior after a long and probably very slow course.

On the whole, then, the conclusion arrived at from the observations made in the tunnel tend to show that geological speculations as to the existence of free currents of water in the earth's interior, and especially of heated water as connected with metamorphic action, are not altogether correct. At a depth of 5,000 feet the temperature of water in the earth's interior should, according to these views, be very high, and the quantity of water should be large. Considering the large size of the excavation, and its great length, much more water than 40,000 gallons per day might be expected to come in, and the

quantity entering might fairly have been expected rather to increase than diminish. Such has not been the case. The water supply that has been reached was very soon exhausted, and the water itself that issued from the fissures intersected was neither very hot, nor was it loaded with mineral matter, nor has it increased in quantity. It can hardly be said to have been in circulation. Thus, so far as metamorphic rocks are concerned, the metamorphosis seems to have been carried on away from water action of any kind.

The temperature of rock in the earth's interior is another enquiry of very great interest and importance. It is to be regretted that, owing to want of proper arrangement from the first, the method of taking the temperature of the rock has not been fully and properly carried out on both sides. On the Italian side the observations are better than on the French side; but no complete record of either has yet been prepared. The system adopted was to bore holes to about ten feet in the solid rock, at intervals of about 500 mètres, and put in thermometers. But the thermometers themselves were not, in all cases, maximum and minimum instruments, and occasionally accidents have happened. It is understood, however, that further observations may at any time be made.

The general result, as represented by Signor Borelli, the resident engineer on the Italian side, shows a remarkable uniformity in the temperature of the rock throughout. At the distance of 6,200 mètres (6,506 yards), or nearly midway, and at a depth of about 5,000 feet, the temperature of the rock was found to be only 80°. The mean annual temperature of the surface at the mouth of the tunnel and above is not very closely determined; but, under any circumstances, the increment, compared with that recorded in observations made in mines, is exceedingly small. The greatest depth reached in mines has been 2,150 feet, and the average increment is generally taken at 1° Fah. for every 60 feet. Allowing 80 feet to reach the stratum of invariable temperature, the permanent temperature at 5,000 feet should be at least 80° above the mean annual temperature. It is certainly not more than 50°, and probably less. There have, however, been other cases observed, at which the increment amounted to 1° for upwards of 100 feet; and as there is a general absence of mineral veins and metalliferous deposits of every kind in the rocks through which the tunnel is bored, it may be possible that this smaller increment belongs to the earth generally, the higher being due to chemical action, induced by the presence of certain metals, metalliferous minerals, and water.

At the same time it must be remembered that the very large proportion of all the observations of subterranean temperature

whether made in mines or borings for water, but in no case to half the depth of the central part of the tunnel, shows the rise to be one degree in about 54 feet as an average and the stratum of invariable temperature to be about 60 feet. This is the rate not only when the boring commences near the sea but also near Geneva in a deep sinking commenced 1,600 feet above the sea. The calculated temperature, therefore, in the tunnel under the crest, would certainly be enormously higher than the observed rate. It may perhaps be suggested that something should be allowed for the steep slope of the mountain, but this slope is not sufficient to make the distance from any point of the surface much less than the distance from the crest.

It may be considered, then, that in this matter the result of the observation of temperature of the rock within the tunnel tends to shake confidence in the conclusions hitherto received with regard to the average rate of increase of heat in the interior of the earth. And this is the case in more ways than one. It is hardly possible to imagine a case more strictly average in its general nature than the one before us. The rocks in which the observations have been made are absolutely the same, geologically and otherwise, from the entrance to the tunnel on the Italian side for a distance of nearly ten thousand yards. They are not faulted to any extent, though highly inclined, contorted, and subjected to slight slips and slides. They contain little water and no mineral veins. They consist, to a very large extent indeed, of silica, either as quartz or in the form of silicates chiefly of alumina, and the small quantity of lime they contain is a crystalline carbonate. Such rocks appear to possess no cause that could affect a fair estimate of the rate of increase of temperature. The result, if accepted, would reduce the rate of increment to a degree in about a hundred feet.

It is very much to be regretted that we have not access to all the observations made, and that the character of the observations is not so good as the subject demands. At the same time there is no doubt that the general conclusion is correct, and that the average just stated is a near approximation to a correct one. It is certain that in this tunnel, which affords the best opportunities for close and accurate observation ever obtained, the result is such as to demand a reconsideration of all geological arguments based on the rapid and regular increase of temperature towards the centre of the earth. The temperature of the water already quoted at two points would seem to correspond sufficiently with the observed temperatures of the rock. Thus, in the somewhat powerful spring tapped in 1861 when the work had only proceeded a comparatively short distance, the temperature was 64°, and the superincumbent rock was about 2,500 feet. The rate is also about one

degree in a hundred feet. It was observed with some astonishment by the resident engineer, that for a long distance there appeared no change of temperature, insomuch that he almost ceased to take interest in the observations. During this time, however, the work was progressing under a plateau varying in height from two to three thousand feet above the tunnel and extending for a distance of 13,000 yards from the entrance without much change in elevation.* Shortly afterwards, when the plateau had been passed and when the mountain side rises rapidly, the observations showed a gradual rise, but they have not been reduced and are not at present accessible. It will be important and interesting to know if they show a rise in direct proportion to the increase of height above the tunnel. On the French side the largest spring was tapped at a temperature of about 62° with a superincumbent mass of about 2,000 feet of rock.

A good series of temperature observations on the northern side of the tunnel would have had extreme interest and great value if they had been carried on during the construction with good instruments and due precautions. It is to be feared that this enquiry was to some extent neglected. The holes were bored, and still remain; but it is believed that the temperature of the rock may be to some degree affected by the altered temperature of the tunnel when the works are completed and ventilation is ensured from one end to the other.

There is a difference of level between the north and south ends of the tunnel, the total difference being 435 feet. The Italian end is nearly level, the slope being chiefly on the other side. The contour of the mountain will be understood by reference to the annexed section, but it would be difficult for anyone who had walked over the ground to admit that the grand abrupt mountain pass from Modana and Bardonneche could be so apparently tame and regular as is there represented. The reader will, however, see that it fully exemplifies the above remarks.

The perforation of the Alps under Mont Fréjus has been throughout a remarkably simple and easy operation. There have been no drawbacks of the smallest importance in an engineering sense, and the work has been carried on steadily from the commencement. There has been but a small proportion of hard, tough rock, no loose, treacherous shales, no influx of water, no crushing in of the roof, and no rise of the floor. It is not easy to imagine a more complete instance of plain sailing, or a great work less interrupted by natural or unexpected difficulties. The mere magnitude and novelty of the under-

* This will be at once seen by reference to the section in Plate LXIV., which is drawn to the same scale for vertical and horizontal distances.

taking, and the difficulties anticipated in obtaining fresh air, rendered it to a certain extent a hazardous and speculative matter at one time; but the successful adaptation of the apparatus for boring, by machinery driven by compressed air and carried into the tunnel in such a manner as to be ultimately applied by elastic tubes, completely settled all doubts, and renders it as easy to bore for ten miles, retaining good ventilation, as it had previously been to drive a level for a hundred yards. The geological and physical questions involved were not, however, at first considered, and have been to some extent neglected; but specimens of the rocks cut through have been preserved from the first, and two or three collections of this kind are available. One of these has been taken to Paris by Professor Sismonda, and forms the subject of a description by M. Elie de Beaumont, published in the *Comptes-rendus* of the Académie des Sciences for the 4th July last. Other but less complete collections, with many duplicates, exist in the offices of the resident engineers at Modana and Bardonneche respectively. Both are in the highest degree interesting, and an inspection of them has greatly assisted the author in preparing this article. He has also to express his acknowledgements to the engineers for their kindness in placing before him the results of such observations as they have made on the various subjects alluded to.

It should be known that the present is a very favourable moment for examining the works and their geological results; although the inside of the tunnel is not easily accessible, owing to the great activity with which the works there are being pushed. There is no cessation, night or day, from one year's end to another, the only holidays in the year being on the occasion of the great festivals of the Church. To ensure the greatest activity and energy a premium is given to all hands employed in proportion to the rate of progress, and sometimes it has been found necessary to check the extreme eagerness to get on lest accidents should occur. The accidents hitherto have been very few and slight, and the general conduct of the works reflects the greatest credit on all concerned. The study of the rocks in the neighbourhood, and their comparison with the fragments brought out from the end of the tunnel, will afford ample occupation to the geologist for many days, and will not fail to render him familiar with some of the most interesting results of metamorphic action. The total absence of plutonic rock in the district near the tunnel will not fail also to attract his attention.

The whole group of rocks alluded to in this article belong, as has been already pointed out, to the middle part of the secondary system, from the middle oolite to the Rhætic inclu-

sive. They are called by Italian geologists the *Rocce Antracitifere delle Alpi*, and under this name form a connected group of enormous thickness, repeated in this part of the Alps two or three times by anticlinal and synclinal axes, and generally very highly inclined. These are shut in on both sides by granite and protogine, sienite and diorite, and occasionally penetrated by these latter rocks and by a variety of serpentine and euphotide. The magnesian character of these rocks will be at once recognised, and is seen also in the talcose and steatitic nature of the metamorphosed schists. Although the tunnel does not cut through any of the intrusive rocks, and does not seem even to approach them, it shows very clearly the presence of magnesia as connected with the metamorphosis. This is another matter especially and locally interesting. The great abundance of gypsum, the same in character and appearance in the tunnel and on the surface, is another point to be observed. We leave these matters to the careful study of geologists.

EXPLANATION OF PLATE LXIV.

FIG. 1. Section from N. 14° W. to S. 14° E. on the line of the great tunnel from the mountains on the northern side of the Arc valley, in Savoy, to the mountains between the lateral valley of Rochemotte, to the valley of the Dora, in Piémont. This section is carefully drawn to the same scale of vertical and horizontal distances.

FIG. 2. Plan of the country immediately adjacent to the tunnel, showing the position of the principal valleys on each side the central axis.

N.B.—The following are the rocks as identified by Professor Sismonda, and met with in the tunnel commencing with the north or French entrance.

	Actual thickness.	Distance tunnelled through.
Weathered and fallen rock	200 feet.	420 feet.
1. *Upper member*, consisting of schists, sandstones, and psammite with anthracite, resting on a thick band of quartzite, corresponding to Oxford clay series	4,500 „	7,707 „
2. *Middle member*. Gypsums, crystalline, limestone, and calcareous schist, corresponding to lower oolites	1,700 „	2,815 „
3. *Lower member*. Calcareous schist with bands and strings of quartz and threads of limestone. Liassic and Rhætic series	17,500 „	20,151 „
	23,900	40,093

GREENWICH TIME AND ITS TELEGRAPHIC DISTRIBUTION.

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THE object of our present paper is to describe that system by which Greenwich time, as found by astronomical observation at the Royal Observatory at Greenwich, is daily transmitted, by telegraphic aid, to distant parts of the kingdom; a subsidiary use of the telegraph which, although not directly contemplated in its establishment, yields practical advantages of no small value.

But before directly proceeding to consider its utilitarian applications we must give some account of the manner of reckoning and determining time. The astronomical considerations involved are, however, so fully treated in works on astronomy that we need only concern ourselves here with the more practical aspect of the subject.

When the sun reaches its greatest daily altitude in the heavens we call the time noon, and the interval which elapses between one noon and the next we call a solar day. But the natural solar day thus measured is (owing to the varying motion of the earth in its elliptic orbit, and the inclination of its axis of revolution to the same orbit) to a slight extent variable. Its length oscillates between certain small limits, which renders the ordinary use of such a day for many reasons inconvenient. The inequality is fortunately small as compared with the length of the day, so that its use in practice is avoided by assuming the existence of an artificial solar day—one of uniform length, and consequently better adapted to the wants of mankind. It is known as the *mean* solar day. Natural solar time (that shown by a sun-dial and variously called "true" or "apparent" solar time) is sometimes rather before and sometimes rather after mean solar time (that shown by a clock). Four times in each year they are together. The difference usually existing between them, which amounts to as much as 16 minutes in

the month of November, is the "equation of time" of our almanacs. Its amount for each day at noon to the nearest second of time is contained in common almanacs usually under the heading either of "clock before sun" or "clock after sun:" for greater accuracy reference must be made to the *Nautical Almanac*.

Having shown the relation existing between apparent or sun-dial time and mean solar or clock time, we see how it is that, taking time from a sun-dial and allowing for the equation of time, ordinary clock time is obtained. But a sun-dial is useless for any accurate determination; and of other instruments and methods, giving something more of accuracy, space will not allow us to speak. We must hasten to describe that special instrument, the "transit instrument," which is always employed in fixed astronomical observatories. This instrument consists of a telescope fixed at its centre to a cross axis supported at the extremities on bearings firmly fixed in an east and west position, so that on turning the telescope on its axis it points successively to all parts of the meridian (that imaginary great circle in the heavens which corresponds to the brazen meridian of a celestial globe, and at which the heavenly bodies attain, between rising and setting, their greatest altitude). In order that it may do this precisely, the line of sight of the telescope must be at right angles exactly to the cross axis, and the axis itself must be truly level and also precisely east and west; but no instrument, if placed in exact position, will long remain so. It is therefore usual to register its small deviations, and apply corrections as necessary to the observations. The instrument made use of at Greenwich is the one meridian instrument of the Observatory, the noble transit-circle (designed by the present Astronomer Royal). Such an instrument is used for many purposes besides the determination of time, but it is with this use of it only that we have to do here. On looking into its telescope we see a number of delicate vertical threads across which objects must pass in their transit through the field. The centre thread represents the meridian, the others being uniformly distributed, an equal number on each side. The time at which, by the sidereal clock (always that employed in an observatory), the object to be observed is upon each thread being noted, the mean of the observed times gives a more accurate value of the meridian transit. Usually an observer counts the beats (seconds) of his clock, and estimates the time at which the object is on each thread; but at Greenwich this method is no longer pursued, for by means of the chronograph (brought into use in the year 1854) all transits are registered by galvanism. Of this instrument we cannot here attempt description further than to say, that by its means

the sidereal clock is made to register its seconds by punctures on paper fixed on a cylinder which revolves uniformly, and on which the observer at the transit-circle is able similarly to register any transits he may make. He has only to press a finger-piece attached to the transit-circle to effect the necessary registration. The punctures made by the clock form a scale by which the times corresponding to the punctures made by the observer are easily ascertained. The times for any transit being extracted from the register, and the mean taken, it is further corrected as necessary for the small deviations of the instrument, and finally for "personal equation," that slight constant difference found to exist between even the best observers, by taking account of which observations are reduced to one standard. By this treatment we obtain the clock times of transits such as would have been found had the transit-circle been in perfect adjustment and all observations been made by one person.

Now it is of course possible to observe the sun at noon with the transit instrument and mean-time clock (taking the mean of the transits of the preceding and following borders), and, by aid of the equation of time, infer the error of the mean-time clock. But this is not the way an astronomer proceeds: he refers to the stars. Time can be thereby more accurately determined, and stars may be seen at some part of most nights, whilst the sun will often be invisible at noon for many days together. But the sidereal day differs from the solar day. The length of the solar day depends on the revolution of the earth on its axis and its advance in its orbit round the sun; that of the sidereal day on the revolution of the earth on its axis alone. The consequence is that the sidereal day is shorter than the solar day by nearly 4 minutes of time; and therefore a sidereal clock, or one that completes 24 hours in a sidereal day, must be used. The sidereal day commences when the "first point of Aries" (on a celestial globe one of the points in which the ecliptic cuts the equator) is on the meridian. Mean solar and sidereal time coincide once only in each year, on some certain day in spring. At other times they differ; for as the stars shift once round in a year as regards the sun or solar day, so the sidereal clock in the same period shifts once through the 24 hours as respects the mean solar clock, the relation between the two being very exactly known. The position of any star is known by its right ascension and declination. In a general sense these correspond in the heavens to longitude and latitude as measured on the earth. In determining time we have to do with its right ascension, which is reckoned from the celestial meridian of the first point of Aries. And although the stars are, as it were, "fixed," it is still matter of calculation

to obtain their places for any given time, principally on account of small changes of the celestial planes of reference which astronomers are compelled to use. Suffice it therefore to say, that from the *Nautical Almanac* may be obtained the places of a great number of stars for every day of the year. An observation of any suitable star of this list being made with the transit-circle, and treated as before described, the result compared with the *Nautical Almanac* right ascension gives the error of the sidereal clock, knowing which the error of the mean-time clock can be easily found.

It was always necessary that time should be regularly determined at the Royal Observatory for its own special purposes. In the year 1833, however, an attempt was successfully made to give time by signal to the outer world. A pole, carrying a black ball about 5 feet in diameter, was then fixed on one of the turrets of the ancient portion of the Observatory. Being raised on the pole very shortly before 1 h. (half-way up at 5 m. before 1 h., and full up at 3 m. before 1 h.), the ball is dropped precisely at the instant of 1 h. Greenwich mean time. Its fall is at first rapid (this start is the proper instant to note); afterwards a piston, attached to a rod extending from the ball downwards, entering an open-topped cylinder, the gradual escape of the compressed air so checks its fall that it is terminated quite gently. The ball was for many years dropped by hand (by pressure on a trigger which released the piston), but since the year 1852 it has been dropped by automatic galvanic action, as will be hereafter described.

Before proceeding to speak of the wider distribution of Greenwich time, we must say a few words in reference to local time. This is merely time as determined at any place by astronomical observation. Places north or south of a given place have the same local time; places east or west differ. Suppose, for instance, two clocks, one at Greenwich and one at Bristol, are set right by astronomical observation. Could one then be transported to the place of the other, the Greenwich clock would be found to be about 10 minutes fast of the Bristol clock. Or the sun arrives at the Greenwich meridian 10 minutes before it arrives at the Bristol meridian. It will be thus understood how, when railways began to grow up, it became necessary to employ, for safe regulation of the traffic, one uniform time. Greenwich time, now long known as "railway time," came to be adopted, and its use in the country is now universal.

In course of time the want of some accurate and convenient standard of reference seems to have been felt, not alone for the service of railways, but also for that of the accompanying telegraphs, which so rapidly sprung up. The Astronomer Royal also, viewing the gradual rise of the telegraph system, and

especially the laying of the first submarine cable, became early desirous of placing the Royal Observatory in communication therewith, to be prepared to meet the scientific demands likely then to arise, as well as to be ready to supply any possible public demand for Greenwich time. How at last the time-signal system came to be proposed the Astronomer Royal himself is scarcely prepared to say: it was, as he expresses it, "partly in conversation, partly in other ways; but to Mr. C. V. Walker, Mr. Edwin Clark, Mr. Latimer Clark, and afterwards Mr. C. F. Varley, is the existence of the system due." After some correspondence, the Astronomer Royal, in the year 1852, obtained permission from the South-Eastern Railway Company (on the representation of Mr. C. V. Walker, their telegraphic engineer) to erect wires on their railway, for the purpose of obtaining communication with London; and special apparatus being provided, the system of signalling time from the Observatory was commenced. We shall not, however, further follow the subject historically, but proceed to describe the system as it now exists.

As respects, now, the time-distributing apparatus, the clock specially erected at Greenwich claims first attention. This clock, the normal mean-time clock of the Observatory (erected in the year 1852), is kept adjusted as nearly as possible to Greenwich mean time. It is maintained in action (on Shepherd's plan) by galvanic power alone. When its pendulum (a seconds pendulum) swings to the right, a galvanic circuit is closed, which causes an electro-magnet to raise a small weight. This being discharged on the pendulum in its swing to the left gives it a small impulse, which, repeated at each swing to the left, suffices to maintain it in action. Other galvanic circuits, closed, one as the pendulum swings to the right, another as it swings to the left, allow galvanic currents, alternately positive and negative, to pass to a pair of electro-magnets placed above it. These currents cause the electro-magnets to attract and repel alternately certain bar-magnets, giving thereby a reciprocating motion to the axis which carries them. An anchor on the axis gives forward motion to a wheel carrying the seconds hand, from which, by a simple train of wheels, motion is communicated to the minute and hour hands. So far as concerns the normal clock proper. But if the wire which passes from the pendulum to work the hands is afterwards led (before being returned to the battery) to other electro-magnets in different parts of the building, each pair similarly working hands on a dial, the hands on all will advance together, their forward movement depending entirely on the galvanic current let off at each second by the one pendulum, which consequently governs the whole system. There are within the

Observatory seven such additional dials. One, fixed in the boundary wall, near the entrance gate, for public use, is daily consulted by great numbers of people, and three in the Chronometer Room (two showing seconds only) are used for the rating of the navy chronometers. Of the remainder it is necessary only to say that one (a small one) is placed on the desk of the Superintendent of the Time Department, for a purpose to be yet spoken of. In addition to *driving* these clocks, the normal clock further *controls* (on the excellent principle introduced by Mr. R. L. Jones) several other clocks, situated in London. In controlling clocks the galvanic force acts not as the motive power, but as an auxiliary only, to keep quite right clocks going already very nearly right by their own independent power. Mr. Jones effects this in the following way:—Taking an ordinary clock with seconds pendulum, a galvanic coil is substituted for the pendulum bob, the hollow of the coil being placed horizontal. At each swing the coil encircles permanent bar-magnets fixed to the clock-case. Whilst no current is passing no effect is produced, and the pendulum keeps its own rate undisturbed; but if a current, received at each second of time from a standard clock, is made to circulate through the coil, it becomes for the instant magnetic, and such mutual action arises between it and the bar-magnets that the clock (within certain considerable limits) will, if losing, be accelerated, and, if gaining, will be retarded, the pendulum vibrating in complete sympathy with that of the standard clock.

The normal clock at Greenwich thus drives certain clocks within the Observatory, and controls others in London. But the best clock, when set right, will soon begin to deviate. Means must therefore be provided for easily correcting small deviations. (The way in which the amount of error is found will be immediately described: we speak here only of the manner of correcting it when known.) Now as the whole system of clocks depends on the pendulum of the normal clock, acceleration or retardation of its pendulum will simultaneously accelerate or retard the whole of the clocks. The only consideration to be regarded is that alteration must not be made too rapidly, otherwise the controlled clocks (which are, as it were, *guided* and not *driven*) might not follow the change. The plan adopted for correcting the clocks (one devised by the Astronomer Royal) is the following:—A bar-magnet, attached in a vertical position to the normal clock pendulum, is carried by an arm projecting from the pendulum forwards, it therefore swings with the pendulum. Immediately below, in a central and vertical position, a hollow galvanic coil is supported on a shelf carried by the clock-case. When a galvanic current is thrown into the coil it imparts to the

coil the properties of a magnet, and reversion of the direction of the current reverses the direction of magnetism. If, therefore, a current is passed through the coil such as will cause attraction between the adjacent ends of the swinging magnet and fixed coil, the pendulum will be accelerated. An opposite current, producing repulsion, will retard the pendulum. Either current must remain in action (as at present arranged) ten minutes to change the clock by one second of time. An error of a small fraction of a second can thus be easily and certainly corrected.

The reader will remember that we brought up the description of the manner of determining time so far as to show how, by star observations, the error of the sidereal clock is obtained. Now the sidereal clock works, by galvanic means, a small clock placed on the desk of the Superintendent of the Time Department, near to the small mean-time clock before spoken of. On this desk, then, we have two small clocks—one representing the sidereal clock, the other the normal mean-time clock, both of which are in other parts of the building. And between the two clocks is placed a commutator, by which a direct or reverse galvanic current can be passed through the coil in the normal clock for acceleration or retardation of its pendulum. To ascertain the error of the mean-time clock we take, at any instant at pleasure, by the sidereal clock, the reading of the mean-time clock. Knowing the error of the sidereal clock (we do not correct this error, it being more convenient for astronomical purposes to allow it to accumulate), the true sidereal time of comparison is known. The corresponding mean solar time is then easily calculated. The difference between this calculated time and that read from the mean-time clock is its error, and the error also of the normal clock and all clocks in connection with it. The necessary correction is then made by turning the commutator handle to accelerate or retard the normal clock pendulum for as long a period as may be necessary: the rate at which the alteration is made is mentioned in the preceding paragraph. The normal clock is thus corrected several times daily, always immediately before 10 h. A.M. and 1 h. P.M., for reasons that will be afterwards apparent. The correction required is usually some fraction of a second only.

We have now to speak of a most important duty which the normal clock performs. This is the transmission of galvanic time-signals from the Observatory at the exact instant of each hour. For this there passes through the clock a galvanic circuit; but in the course of the wire there are two breaks: one is united at the instant the seconds hand marks 60, the other from shortly before to shortly after the minute hand

marks 60. So that only at the instant of each hour are both breaks together united, and only then can a galvanic current pass. Each hourly current acts upon two electro-magnets. One is used at 1 h. P.M. only, to discharge the Greenwich time ball. The other has a far more extended use: by relay* action it completes two other galvanic circuits, each giving hourly signals on a separate line of wire. One of these lines is in communication with the Central Postal Telegraph Office in Telegraph Street, London; the other passes to the London Bridge Station of the South-Eastern Railway. Hourly time-signals pass from the Royal Observatory along these lines day and night, and with the efficient performance of this duty the special responsibility of the Observatory terminates, the further distribution of the signals thus transmitted being under the control of other parties. [In speaking of "currents" and "signals," proper distinction cannot always be preserved, although, strictly speaking, by "current" we understand the unseen something which conventionally is supposed to pass along a wire; by "signal" the effect which the unseen current produces upon a telegraph needle or other indicator.]

The use made of the hourly signals in each of these lines must be considered separately. Taking that first mentioned, we proceed now to explain that in the Central Telegraph Office there is fixed an admirable and elaborate apparatus, designed by and constructed under the superintendence of Mr. C. F. Varley, engineer to the then existing Electric and International Telegraph Company, for the purpose, as he himself explains, "of sending exact Greenwich time simultaneously and automatically to numerous local and provincial stations." The whole collection of apparatus is known as the "Chronopher." It acts as a gigantic switch and relay, and by its means the one Greenwich current is transmitted on many different lines. These lines are, for convenience, disposed in two groups—one consisting of wires passing to points in London, the other of wires extending to distant parts of the kingdom, including such places as Manchester, Birmingham, &c. For the service of these groups of wires there are two relays—one the "local" relay for the London group, the other the "provincial" relay for the country group. On these relays only does the Greenwich time current act. In the local relay it causes a current to pass away simultaneously on each branch of the London group of wires; similarly in the provincial relay it causes a

* In its simple form the relay is an instrument which, on receipt of a galvanic current—which may be from a distance—completes the circuit of a battery of its own, either for transmission of a signal to another station, or for the performance of some mechanical work.

current to pass away simultaneously on each branch of the provincial group. There is, however, this difference: the distribution in London takes place at every hour, the wires being used for time-signal purposes only; but for the country, as it would at present be too expensive to employ special wires, those of the ordinary telegraphic service are used, general distribution of time on these wires being made once each day only, at 10 h. A.M. Every care is taken that this distribution shall be effective and certain. The chronopher includes arrangements by which (by means of a clock) the various provincial wires are, immediately before 10 h., automatically disconnected, each from its particular speaking instrument, and placed in communication with the chronopher commutator, in readiness for the Greenwich current. When it has passed and distributed time currents throughout the provincial lines, the ordinary connections of the lines are again automatically restored. And amongst minor arrangements there is one for preventing, almost entirely, interruption from accidental galvanic current arising, in any way, in the Greenwich wire. These 10 h. currents, being distributed along the principal lines of railway, give time daily through a large extent of country; they are used, to a considerable extent, for the direct correction of railway clocks, and indirectly rule them all, these clocks acting again each as a standard for the clocks of the neighbouring districts.

In some instances currents distributed from the Central Telegraph Office have been used for giving time to the public by public signal. In the year 1863 the River Tyne Commissioners promoted the establishment of two such signals, which have proved to be of great value. A 12-pounder gun at Newcastle and a 24-pounder gun at North Shields have, since the year mentioned, been fired daily at 1 h. P.M. Greenwich time. At London the chronopher, by automatic action, places the Newcastle wire for a short time in proper state to be acted on by the Greenwich 1 h. current; at Newcastle proper changes of the speaking circuits are there also automatically made. As soon as at 1 h. the seconds hand of the Greenwich normal clock marks 60, the relays at Greenwich, London, and Newcastle will each successively act, and galvanic currents will pass to the guns and fire them before the sound of the clock beat at Greenwich has well died away. The fuse used for the guns is Abel's chemical fuse, which explodes on passage through it of a galvanic current. Two fuses are inserted daily at each gun, to avoid failure should one of them by accident miss fire.*

* In taking time by the *sound* of a gun it is of course necessary to allow 4½ seconds for each mile that the gun is distant from the observer.

It should be further mentioned, as respects the distribution of time in London, that hourly signals are received at the Post Office; and also at the Westminster clock, for facility of regulating it; the clock gives also return signals to Greenwich, by which its error is there known. As the clock is not controlled or in any way acted upon by galvanic current, it may deviate slightly from true time; but its deviations are small, and seldom amount to more than two seconds of time.

We have now to consider the use made of the hourly signals which pass from the Royal Observatory in the line terminating at the London Bridge Railway-station. These signals, with the exception of that at 1 h. P.M., are placed at the disposal of the South-Eastern Railway Company, who, in return, accord to the Royal Observatory the necessary wire communication at 1 h. for an important special purpose. Mr. Walker, to whom the establishment of these relations is due, distributes the signals received by him on the lines of the South-Eastern Railway, principally for regulation of the station clocks, &c. We have before us his *Table of the Distribution of Time Signals*, showing how extensively and regularly the daily work of distribution is done. Many signals are sent by hand, but some are accurate transmissions of the Greenwich current itself, and these are managed as follows:—At London Bridge a clock (one of those controlled by the Greenwich normal clock) acts the part of an automatic switch. At each hour it, as it were, turns some railway or other wire on to the Greenwich wire, so that at different hours the signal is transmitted in a different direction. The special service accorded to the Observatory at 1 h. consists in the giving the necessary wire communication for enabling the 1 h. current from Greenwich to pass directly to Deal, to discharge a time-signal ball (similar in principle to that at Greenwich) placed on the old semaphore tower. The Greenwich current, by relay action at Deal, drops the ball, which, whilst falling, makes such momentary changes of wire connections as causes a signal (the "return signal") to be received at Greenwich, showing there that the ball has really been discharged. This ball was erected by authority and at the expense of the Admiralty, to give time to shipping in the Downs, and it has been in use since the year 1855.

The Deal ball signal is of special use as giving to masters of ships the means of obtaining the errors and approximate "sea rates" of their chronometers; the rates of chronometers often changing when placed on shipboard. Other such signals might be very usefully established at important points of our coasts. The Astronomer Royal has himself urged on the attention of the Government the desirability of establishing hourly time-signals at the Start Point, and the Ship-Owners' Association of

Liverpool have likewise made enquiries as to the facilities for exhibiting an hourly time-signal on the Tuskar Rock. These points, in the English and Irish Channels respectively, are both advantageous for such signals ; but some special wire communications would in both cases be necessary, so that the first outlay might be rather large, otherwise there would be no practical difficulty.

We have now completed our account, which intentionally has been confined to the time-operations in connection with the Royal Observatory at Greenwich. In conclusion we may however remark, that the local observatories in Liverpool, Edinburgh, and Glasgow have also for many years done much to give authoritative Greenwich time in their respective surrounding districts. In Glasgow there is a most extensive system of controlled public clocks, and Edinburgh and Liverpool both possess time guns.

THE ECHINUS, OR SEA-URCHIN.

By St. GEORGE MIVART, F.R.S.

[PLATE LXV.]

THE Englishman, unacquainted with natural history, who for the first time visits Marseilles will, when he wanders down to its busy port, most probably have his curiosity awakened by baskets full of dark, round, spiny bodies (disclosing deep yellow parts within), each about the size of an egg. These are the sea-urchins, sea-eggs, or *echini*, which are largely affected by the good folks of Marseilles, and constitute one of the many objects of their fish-market which interest, surprise, or disgust the northerner on his first arrival on the Mediterranean shore.

A large specimen of echinus well indeed merits its name of sea-urchin, for externally it presents an amazing resemblance to a rolled-up hedgehog, or urchin, being covered over with spines which in size and general appearance are very singularly like those of the last-named animal.

The resemblance, however, between these two animals is of the most superficial character only, and two creatures more really distinct could hardly be selected from the whole animal kingdom.

The sea-urchin presents us with a singular mixture of great simplicity of structure united with very great complexity. It is indeed an animal formed on a very low type, which, while strictly preserving that low type, yet preserves it in a wonderfully ornate condition with a quite prodigious number of complications and adornments.

The creature, when deprived of its spines, presents the appearance of a spheroidal, melon-shaped body (the so-called shell), furnished with two poles (each being provided with an aperture), and with lines, like meridians, running from the vicinity of one pole nearly to the opposite one. The shell when thus stripped is seen to be formed of a multitude of parts, to be covered with small rounded prominences, or tubercles, and to be perforated by a vast number of minute holes, or foramina.

The whole structure will at first naturally be thought to be an external skeleton, like the true shell of a mussel,* or the hard investment of a lobster.† It is, however, nothing of the kind, but is a truly internal skeleton, inasmuch as the soft substance of the animal (the *perisoma*) coats it externally as well as internally.

The numerous, distinct, calcified plates which form the shell are arrayed in a very definite order. Of the two poles the inferior one is termed the "oral pole," as it is there that the mouth is to be found. The superior one is called the "apical pole," and this is the situation of the anus. Around the anus are placed some small and more or less irregular plates, termed "anal plates." External to and immediately below the anal plates are ten large ones, each perforated, and forming together a single circular row. These ten plates are the "ocular" and "genital" plates, and they alternate with each other so that each ocular plate is between two genital plates, and each genital plate is between two ocular ones. The perforation in each genital plate is the external opening of a genital duct. The perforation in each ocular plate is for an eye-spot. Below this circle of ten plates is the great bulk of the shell, which is called the "*corona*," and consists of five vertical tracts beset with perforations and termed the *ambulacra*. Alternating with these are five other tracts of greater width, destitute of pores and termed the *interambulacra*. Each ambulacrum and each interambulacrum is narrower at its apical than at its oral end, and consists of two vertical series of plates. Each of these plates is pentagonal, while two angles of each pentagon are right angles. A zigzag suture joins together the two vertical series of ambulacral plates in each ambulacrum, and a similar suture unites together the two vertical series of interambulacral plates in each interambulacrum. On the other hand, a straight suture unites the flat margins of each series of ambulacral plates with the flat margins of the adjacent series of interambulacral plates. The plates which form the ambulacrum are seen, when closely examined, to consist each again of three pieces (*pore-plates*), which form between them the six pores, or foramina, of each ambulacral plate.

The interambulacral plates, on the other hand, are single, and without perforations.

There are never more than two vertical series of plates in each ambulacrum and in each interambulacrum. Therefore there are five pairs of rows of ambulacral plates and five interspaces occupied by five pairs of rows of interambulacral plates.

* See the article on "The Anatomy of the River-Mussel," by Mr. J. C. Galton, POPULAR SCIENCE REVIEW for July 1870.

† See the article on the Lobster in the same work for October 1868.

Thus there are altogether twenty vertical (or longitudinal) series of plates, ten of these being ambulacral and ten being interambulacral.

The relation of these alternating tracts to the circle of ocular and genital plates is such that an ocular plate is placed at the summit of each ambulacrum, while a genital plate stands at the top of each interambulacrum.

One of the five genital plates is larger than the rest, and has a porous space, which gives a worm-eaten look to its surface. This modified genital plate is termed the *madreporic tubercle*, and is of course, like the other genital plates, interambulacral in position, or, as it is sometimes called, *inter-radial*.

At the opposite, or oral, pole (beyond the corona formed of the ambulacra and interambulacra) are the *buccal plates*, which are small and irregular and are scattered in the buccal membrane which surrounds the oral aperture.

All these plates forming this singularly composite shell consist each of a network of calcareous spicula formed in the soft perisoma, and they thus have the same relation to the flesh of the echinus that the skeleton of a coral has to its investing body. The spicula meet and unite at right angles, and often form very definite patterns.

In the living state the shell is covered with spines, each spine being articulated to one of the tubercles before spoken of. The articulation is effected, at least in the larger spines, by means of a ligament which passes from a little pit on the surface of the projecting tubercle to another little pit situated on the convex lower end, or base, of the spine, like the *ligamentum teres* of our own hip joint. Thus great freedom of motion is allowed, while the motions themselves are effected by means of small muscles which extend from the shell around the tubercle to the projecting parts of the base of the spine. These muscles are, of course, invested by the external layer of the soft perisoma of the body. The spines are important organs of locomotion, and are the agents by which burrowing in the sand is effected. Locomotion, however, is also aided by tubular suckers, which abound in each ambulacrum. Each sucker springs from two pores, a minute tubular vessel passing out through each pore, and then each pair of such minute vessels joining together to form one sucker. On the other hand, the two minute vessels unite again within the shell, to form a saccular dilatation, which is directly continuous with the ambulacral system of vessels to be presently described.

Each sucker is mostly cylindrical, but is flattened and enlarged at its free end into a disk. This disk even is furnished with five flattened calcareous pieces, of about equal size, together forming the *rosette*, and the stalk of the sucker is often beset

with spicula. These suckers, being very extensible, can be protruded and withdrawn; and by their aid locomotion is indeed mainly effected.

There is a third and most singular group of organs scattered over the whole surface of the buccal membrane and corona. They are of minute size, so that to observe them requires a handglass, and they were formerly taken to be parasites. They are termed *pedicellariæ*, and are complex organs, being even each furnished with a delicate internal skeleton. Each consists of a long slender stalk, ending in three short limbs (or jaws), which diverge from the distal end of the stalk. These jaws open and shut with a snapping action, while the stalk sways about.

The utility of these organs is not yet fully determined, but they have been observed to remove particles of excrement, passing them along, as it were, from hand to hand. They may, however, also pass along nutritious particles to the mouth, and they may serve to detach foreign bodies, ova, &c., which, if allowed to remain, would grow up parasitically and injure the echinus. They singularly remind us of the bird's-head processes of certain polyzoa or bryozoa (polyp-like animals, with ciliated tentacles).

On the inside of the corona, at the lower end of each ambulacrum, is a solid calcareous arch, termed an *auricula*, while within the space bounded by these five auriculæ is that singularly complex masticating organ termed "*Aristotle's lantern*," made up of a score of distinct parts. When, in the unmutilated animal, the mouth is looked at, the apices of five pointed teeth are seen to protrude more or less.

Each of these teeth is externally very like a cutting tooth of a rat or squirrel, though much simpler in minute structure. It is constantly reproduced from a soft root at the upper end (where it is enclosed in a secreting bag) as it is worn away at the apex.

Each tooth is enclosed in a composite* vertical plate, folded on itself and interambulacral in position. These are the *alveoli*.

The adjacent upper edges of the alveoli are held together by strong horizontal pieces, which pass from one alveolus to the next, are oblong in shape, and are called the *rotulæ*. Each rotula is opposite an auricular arch.

The last set of pieces are the five *radii*, which are long slender parts fixed to the apices of the rotulæ like springs, extending upwards and outwards, and bifurcating at their ends.

All these complex parts act together, and separate and ap-

* "Composite" because it consists, primitively, of four parts.

proximate the apices of the five teeth with ease, force, and delicacy.

The muscles by which these movements are effected, and which, like those of the lobster, are formed of striated fibres, are—1. Those which connect the adjacent sides of the alveoli, which are finely striated to give insertion to these muscles. 2. Those which pass from the auriculæ to the apices of the alveoli—i.e. from those arches before mentioned within the cavity of the shell at the base of the ambulacra. The two muscles of each pair of this category pass from one auricula to two alveoli, each alveolus receiving one such muscle from the auricula on each side of it. 3. Those which pass from the bases of the alveoli to the interspaces of the auriculæ. 4. Those which connect together the radii; and, 5. Those which pass downwards from the apices of the radii to the inside of the shell in the interspaces of the auriculæ—one muscle passing from one arm of the bifurcation, to the shell on the same side of the adjacent auricula; the other muscle passing from the other arm of the bifurcation, to the shell on the other side of the adjacent auricula.

The muscles of the categories 3 and 5 approximate the teeth; those of the category No. 2 separate them.

In the angles opposite the notches of the oral margin are placed five branched gills, or plumose external processes of the perisoma, attached to the buccal membrane.

Internally the mouth leads to an alimentary canal of a very simple character, without any differentiation into stomach and intestine; the œsophageal part, however, is narrower than the rest. This simple canal passes twice round the interior of the shell, to the inner wall of which it is suspended by mesenteric filaments. It terminates in the opening at the apical pole. There is no liver or any other similar organ to aid the process of alimentation.

The cavity of the body external to the alimentary canal contains the true blood, mixed with more or less water. This is kept in motion by means of vibratile cilia, which coat the inner wall of the shell and the alimentary canal.

There is no true heart, and there are no true blood-vessels; but there are many vessels of other kinds to be described directly.

The only generally recognised organs for respiration are the fine plumose processes round the mouth before mentioned; the influence of these, however, must be exceedingly slight.

The vessels containing fluid are of two kinds: one of them is called the *ambulacral system*, the other is termed the *pseudhæmal system*, and has been mistaken for a system of true blood-vessels.

The ambulacral system consists of a circular vessel round the gullet, with five pyriform cæcal processes (termed *polian vesicles*), and giving off five longitudinal vessels. Each of these longitudinal vessels passes up between the adjacent alveoli, up under the corresponding rotula, and down through the adjacent auricula, and finally ascends inside the middle of an ambulacrum, terminating blindly at its summit. Each longitudinal vessel gives off transverse branches, one opposite to each pair of pores, which become continuous with the saccular dilatations before mentioned at the inner ends of the tubular suckers. These ambulacral vessels are ciliated internally, and are kept in relation with the surrounding medium by means of a straight axial canal, which ends above at the before described madreporic tubercle with its sieve-like perforations.

The second or pseudhæmal system of vessels consists of two circular vessels (one round the rectum and one round the gullet) connected together by a dilated vertical vessel, said to be contractile, and also of two other vessels which accompany the alimentary canal.

The nervous system has in its distribution much similarity with the ambulacral system. It is made up of a pentagonal ring, round the gullet, with five, hardly distinct, small ganglia, one at each angle of the pentagon. From each ganglion a very considerable nerve arises, and passing through the auricular arch and between the alveoli, runs up to one of the five eye-spots, where it enlarges and terminates. Each nerve as it ascends gives off lateral branches to the suckers.

The nervous system is more superficial in position than are the vessels. Thus each nerve which runs up each ambulacrum is placed on the outer side of the similarly extending ambulacral vessel.

The eye-spots are the only organs of sense known.

The reproductive system is exceedingly simple. The sexes are in distinct individuals, and in each there are five pairs of glands —ovaries in the one case, testicles in the other. In each case they open externally by the apertures in the genital plates. The ovaries are a favourite article of food in Southern Europe; they are of a bright orange colour when ripe.

The process of development is extremely remarkable, and utterly different from anything met with in any of the typical animals yet described in the POPULAR SCIENCE REVIEW.

After the process of yelk division has taken place the ovum becomes an oval body, with an external, blastodermic layer, apparently made of coalesced particles of the results of yelk division. In this stage the embryo is covered over with external vibratile cilia. Soon a depression appears on one side of the body, and this is the future anal aperture of the larva. The

cilia next become restricted to a single band round the ovum, and at the same time the depression extends inwards, forming a complete alimentary canal, with mouth, pharynx, stomach, short intestine, and anus. The plane of the ciliated band does not coincide with the direction of the alimentary canal, and both are bent down anteriorly. The larva is thus, at first, quite bilaterally symmetrical, i.e. it is divisible into a right and a left half, which are equal and corresponding one to the other. In the adult, as we have seen, the symmetry is radial, not bilateral. The next modification is the outgrowth of the ciliated band into long processes, in which condition the larva is called a *pluteus*, and is furnished with a delicate and complex skeleton of anastomosing calcareous rods. As yet, however, we have no trace of the future echinus. This complex organism, so well furnished with a digestive apparatus, so actively locomotive, and supported by so beautiful a skeleton, is not destined for permanent existence, nor can it reproduce its kind. It is but, as it were, the nurse of the secondary and true larva, to which it is compelled to yield an important part of its own organisation, and the perfecting of which is its own inevitable destruction.

Soon on one side of the stomach of this unlucky primitive larva there arises a mass of formative substance, or bud, and at the same time a depression appears at a point of the external surface, which deepens, becomes ciliated internally, and forms a circular canal at its lower end, and this sends out five other canals around and amongst the bud of formative substance just mentioned. These five canals are the future five longitudinal, ambulacral vessels, and the developed "bud" becomes the young echinus. The rest of the structure of the "pluteus," or primitive larva, withers, shrivels, and dies, its stomach having been enclosed and cut off from the gullet by the secondary larva (or young echinus), which develops its own mouth, intestine, and anus, together with spines, suckers, pedicellariæ, &c.

The further growth of the echinus, until it attains its extreme size, is also effected by means quite different from those employed by the typical animals already described.

The lobster casts its dense coating, the nautilus forms new chambers, the mussel fresh internal layers of shell substance, but in the echinus each separate plate is capable of individual enlargement by means of the perisoma which invests it on all sides; while absolutely new plates are developed as required towards the oral and apical poles of the enlarging corona.

The echinus may serve as the type of a large and important group of animals, consisting, besides the sea-urchin and sea-eggs, of star-fishes, brittle-stars, sea-cucumbers, and crinoids or

sea-lilies, which, with a number of extinct forms, constitute the class *Echinodermata.*

All these forms agree together in having—

1. Calcareous particles in the integument.
2. A system of ambulacral vessels.
3. A nervous system in the form of a ring round the gullet, with nerves radiating from it.
4. Reproduction by means of a more or less roundabout process, i.e. by means of a more or less distinct secondary larva.

They all differ from the lobster and the creatures like it, which belong to the annulose primary division of the animal kingdom, in that there are—

1. No articulated limbs.
2. No circulating system of true blood.
3. No external skeleton.
4. Not only no portal system, but no liver.
5. No auditory organ or organ of smell.
6. No chitinous envelope to the body.
7. No known renal organ.
8. No brain.

They agree with such in that—

1. There is no solid internal structure separating the nervous centres from the alimentary canal.
2. There are no jaws or visceral clefts.
3. That in development the embryo does not present a longitudinal groove.
4. That the anterior part of the alimentary canal is surrounded by the central part of the nervous system.

The *Echinodermata* differ from the cuttle-fish and its allies, i.e. from the *Mollusca*, in that—

1. The nervous system is not disposed in three pairs of ganglia.
2. There is no liver.
3. No limbs.
4. No jaws.
5. No circulating system of true blood.
6. Never an external skeleton.
7. No auditory organ or organ of smell.
8. No known renal organ.
9. No brain.

They agree with such in that—

1. There is no solid internal structure separating the nervous centres from the alimentary canal.

2. No articulated limbs or visceral clefts.
3. That in development the embryo does not present a longitudinal groove.
4. That the anterior part of the alimentary canal is surrounded by the central part of the nervous system.
5. That the body has a calcareous protection, and not a hard chitinous envelope.
6. That it does not consist of a longitudinal series of similar segments, either internally or externally.

The question to what other animals the echinoderms are most nearly related, or whether they are so distinct as to merit the attribution to them of a distinct primary division of the animal kingdom, is a difficult and vexed question.

All things considered, it seems probable that their nearest allies are to be found in certain of the lower worms, such as those amongst which the tapeworm is classed, and which together form the compound and somewhat unsatisfactory group the *Scolecida.* Only in these, besides the echinoderms, is found that complex and singular mode of reproduction which consists of the development of a primary larva as a suitable nidus for the development of the future adult form. Moreover, many of these *Scolecida* present us with a double set of vessels, which appear to answer to the ambulacral and pseudhæmal systems of the echinus; there being in many one set not contractile, but ciliated internally (answering to the ambulacral vessels), and another set which are contractile, but not ciliated internally (answering to the pseudhæmal vessels). On the whole, then, if the echinoderms are associated with any other animals, it will be with the *Scolecida.* This has been done by Professor Huxley, who has proposed the name *Annuloida,* to designate the whole formed by the union of the echinoderms and *Scolecida* in one great group.

As to the subdivisions of the class *Echinodermata,* the existing forms are easily separable into five groups or orders. The first of these is termed *Echinidea,* and comprises all those echinoderms which in their organisation most closely resemble our type—the echinus. The common characters of this order are—that the body is spheroidal, or at the least discoidal; that the integument has so many calcareous plates as to form a shell; that there is invariably an anus; that the primitive larva has the form of a *pluteus,* as just described; and, finally, that the ambulacra extend over the greater part of the shell, or, in more technical language, that the ambulacral region is in excess of the antambulacral region.

The second order (fig. 6) is the *Holothuridea,* which may be described as echini with the integument softened and nearly

dissolved, and the body drawn out at the poles. These are "sea-cucumbers." These have a vermiform body, a flexible integument with calcareous particles, an anus which is terminal in position, and an ambulacral region greatly in excess of the antambulacral region. The primitive larva, however, is, in this group, not in the form of a pluteus, but vermiform. Moreover, sometimes there are but three rows of ambulacra instead of five, the lantern is represented only by rudimentary alveoli and rotulæ, there are no spines and no pedicellariæ, and the generative organs are unsymmetrical, consisting of a bundle of blindly-ending tubes, opening on one side of the neck by a solitary aperture. This order contains the only monœcious form of the class—namely, the genus *Synapta*.

The third order (fig. 7) is named *Asteridea*, and contains the star-fishes. Here the body is mostly stellate, though sometimes it is discoidal. The so-called arms are really parts of the body, into which extend sacculated processes of the alimentary canal. The ambulacra lie in deep grooves on the under surface of the so-called arms, and only on their under surface; and thus the ambulacral region is only coextensive with the antambulacral region, instead of being in excess of it. The integument is strengthened by thick and strong calcareous opicles. The primitive larva is vermiform, and the so-called arms are sometimes more than five in number. In many there are pedicellariæ, but these have only two jaws. There is no dental apparatus in the mouth. The madreporic tubercle is interradial and conspicuous, and its canal, sometimes hardened and jointed, is termed the "sand canal." The nervous system is essentially like that of the echinus, and the diverging nerves are placed superficially to the ambulacral canals, and terminate each in an eye surrounded by movable spines.

The fourth existing order consists of the *Ophiuridea*, or "sand stars" (fig. 8). These animals have real arms, distinct from the body (which is termed the *calix*), sometimes branching and sometimes provided with lateral processes. The ambulacral region is coextensive with the antambulacral region. The integument is calcareous, but not provided with pedicellariæ. There is no anus. In one important point these sand stars resemble the echini, namely, in having the primary larva in the form of a pluteus. The ambulacral vessels run along just beneath the ventral surface of the arms, but here (unlike the star-fishes) they are sheltered within the skeleton, not merely placed in grooves.

The last and most aberrant order (fig. 9) of existing echinoderms consists of the *Crinoidea*, all of which pass at least the early stages of their existence rooted on a stalk. Such forms abounded in that vast period during which primary and secondary rocks

were deposited, and their remains are known as "stone lilies." Now, however, they form an insignificant fraction of the existing representatives of the class, and the permanently stalked form (*pentacrinus*) is very rare, and confined to hot climes alone. These crinoids have the arms stellate, mostly branching. The ambulacral region is coextensive with the antambulacral region, the calix is distinct from the arms, the integument is calcareous, the generative organs are numerous and external, and the primary larva is vermiform. The position of the body is the reverse of that in the echinus and star-fish, the mouth being upwards. Peculiar processes, termed cirri, project round the base of the calix in the free form *comatula*, and from the stalk, at intervals in the fixed and permanently stalked form *pentacrinus*.

It is difficult to obtain good specimens of the *Holothuridea*, because these creatures have the singular habit, when alarmed, of "starting" so violently as to eject the whole of their viscera. Certain of the branching forms are also exceedingly difficult to procure in a perfect state, from the extreme facility and readiness with which they spontaneously break themselves up when captured. The late Professor Edward Forbes gives a very amusing account of his unsuccessful attempt to secure a "brittle-star" which he had caught in his dredge, and for which he had prepared a bucket of fresh water to kill it instantly, and so, he hoped, avoid its demolition. He says: "As I expected, a *Luidia* came up—a most gorgeous specimen. As it does not generally break up before it is raised above the surface of the sea, cautiously and anxiously I sank my bucket to a level with the dredge's mouth, and proceeded in the most gentle manner to introduce *Luidia* to the purer element. Whether this cold element was too much for him, or the sight of the bucket too terrific, I know not, but in a moment he proceeded to dissolve his corporation, and at every mesh of the dredge his fragments were seen escaping. In despair I grasped the largest and brought up the extremity of an arm, with its terminating eye, the spinous eyelid of which opened and closed with something exceedingly like a wink of derision."

DESCRIPTION OF PLATE LXV.*

FIG. 1. An echinus with the spines removed from one half of the shell so as to show—*a*, the ambulacral plates with their pores; *i*, the interambulacral plates; *b*, tubercles for the attachment of spines.

* These figures are taken by permission from specimens which form part of Professor Flower's educational series in the Museum of the Royal College of Surgeons.

FIG. 2. Inside of the shell of an echinus to show—*L*, Aristotle's lantern; *an*, an auricula; *a*, ambulacra; *i*, interambulacra.

FIG. 3. Oral pole of an echinus, showing the apices of the five teeth protruding from the mouth.

FIG. 4. Apical pole of an echinus—*a*, ambulacra; *i*, interambulacra; *g*, one of the five genital plates; *m*, madreporic tubercle, and modified genital plate; *o*, ocular plates.

FIG. 5. Parts which compose Aristotle's lantern; *al*, alveoli enclosing *t*, teeth; *r*, *rotulæ*; *d*, radii.

FIG. 6. *Cucumaria*, one of the *Holothuridea*.

FIG. 7. *Uraster*, one of the *Asteridea*, showing the oral surface and the ambulacra in grooves in the so-called arms.

FIG. 8. One of the *Ophiuridea*: the oral surface.

FIG. 9. *Comatula*, one of the *Crinoidea*: side view. The mouth turned downwards; *c*, cirri surrounding the base of the calix.

THE SUN'S CORONA.

By RICHARD A. PROCTOR, B.A., F.R.A.S.

Author of "Other Worlds than Ours," "Half-hours with the Telescope," &c. &c.

DURING the approaching total solar eclipse the sun's corona will undoubtedly be the chief object of the attention of observers. Hitherto, during great eclipses, the corona has been an object of but secondary—scarcely even of secondary—interest. Even during the American eclipse of last year, the prominences were scrutinised far more carefully than the corona. But next December all this will be changed. For the first time in the history of astronomy, the chief object of the observers of a total eclipse will be the determination of the true nature of this striking appendage; and undoubtedly such observers as journey to Spain or Sicily to view the eclipse will regard their work as a failure, unless it enables them to solve some at least of the problems presented by the solar corona.

On this account I think a brief consideration of the nature of those problems cannot fail to be of interest, while I am not without hope that the study of some of the facts which I am about to adduce may suggest modes of observation or research which may be applied successfully next December by those who are fortunate enough to view the coming eclipse.

It may seem, perhaps, to some that the very circumstance that a great eclipse is approaching, during which many skilful observers will study the phenomena of the corona, should serve to check all attempts at theorising. But as a matter of fact, the whole history of scientific progress shows how important it is that observation should not only be accompanied and followed, but in part preceded, by a process of inductive reasoning. More especially is this the case where the observation is to be made in the course of a few brief moments. Hurried as an observer of a total eclipse must necessarily feel, and startled, too, by the grandeur and solemnity of the phenomena taking place before his eyes,

it is most unfit that the consideration of what is worthiest of observation should be left to the very moment at which observation is to begin. It cannot be but that a careful consideration beforehand of the probable nature of the phenomena he is to observe, of the circumstances which, if carefully noted, may resolve doubts, and of the special parts of the heavens to which his attention should be directed, will tend materially to increase the value of his observations.

Indeed, it is only necessary to consider the records of former eclipses to see that this must be so. Without in any way slighting the observing powers of those who have handed down to us the records of great eclipses, it is impossible not to feel, as we consider what they actually accomplished, that they might readily have accomplished much more. We find the attention of each observer distracted between a variety of objects; facts are recorded which do not tend, and could not possibly tend, to elucidate any of the questions of interest which have been at issue; and, in fine, one record after another displays evidence of an eager anxiety to ascertain new truths, marred by a very imperfect recognition of the way in which that purpose could best be accomplished.

In the first place, it will be well to enquire what lessons may be educed from observations already made upon the corona, and further, what light other observations or researches may throw upon our subject.

There are three theories of the corona which have at various times been upheld by astronomers, and between which it will be well that we should endeavour to make a selection.

These theories assign to the corona very different positions in space. One places the corona around the sun, the second around our moon, the third in our own atmosphere. According to the theory (of these three) which may be finally established, we shall have three very various degrees of magnitude and importance to assign to the corona. If it is a solar appendage, its extent exceeds that of any body within the solar system, save perhaps one or two of the most remarkable comets. If it is a lunar appendage, it sinks into relative insignificance, but still has an absolute volume far exceeding that of our own earth. If, lastly, it is brought within the confines of our own atmosphere, it is not merely reduced to proportions altogether insignificant, as well absolutely as relatively, but it no longer has any real existence as a substance of any sort, any more than the beam of light which shines through clouds can be regarded as an actually existent measurable mass.

I shall take the second of these theories first in order, because it is the one we can most readily dispose of.

To Halley it seemed an acceptable theory, that the corona is

due to the illumination of a lunar atmosphere. Newton and others of Halley's friends held a different opinion on this point, and Halley was therefore unwilling to insist on his theory; but there can be no question that he accepted it as highly probable. But in our day this view has fallen wholly into disrepute, or rather it had done so long before our day. The careful study of the occultation of stars by the moon has convinced astronomers that the moon has no atmosphere of appreciable extent, certainly none so extended as would be required to account for the corona. The study of the cusps of the new moon has confirmed this view. Schröter and others have detected or suspected signs of a prolongation of the cusps, such as would occur if the moon had an atmosphere, and, therefore, a twilight-circle of appreciable extent. But the prolongation is so slight as to prove—even if accepted as existent—that any lunar atmosphere must be of the most limited extent. And, lastly, the most delicate of all the observations which have been made to determine whether the moon has an atmosphere is undoubtedly that which Dr. Huggins made several years ago upon the spectrum of a star occulted by the moon's dark limb.. It is obvious that the chance of recognising in the spectrum of a star the signs of the existence of a lunar atmosphere, causing a gradual diminution of brilliancy as the moon traversed the star's disc—sudden as the passage undoubtedly is—was far greater than that of noticing a change in the brilliancy of the star itself, seen directly. Yet Mr. Huggins could detect no sign whatever which indicated the existence of a lunar atmosphere.

Even regarding the general question of a lunar atmosphere as undecided, we can yet feel no hesitation whatever in regarding it as demonstrated that the moon has no atmosphere which can account for the corona.

Taking next in order the theory that the corona is an optical phenomenon due to the passage of the sun's rays through our own atmosphere, we have considerations of less simplicity to deal with; but yet I think we shall have very little difficulty in coming to a conclusion respecting this theory also.

In the first place, let us endeavour clearly to recognise what the theory is. This is not so simple a matter as it might seem, for those who hold the theory generally that the corona is an optical phenomenon are not always in accord as to the way in which the optical phenomenon is produced. Some, indeed, will not allow our atmosphere to have any part in the matter at all; but while denying that the corona is a solar appendage, or that it is due to the existence of a lunar atmosphere, yet divide between the sun and moon their theory of the cause of

the phenomenon; others regard our atmosphere as alone in question; while yet others consider that, while the illumination of our atmosphere is in question, the moon is concerned in causing that atmosphere to be illuminated.

As regards the purely optical theory, according to which the moon's action on the solar rays is alone concerned, it is to be remarked that the only form of the theory which has been held worthy of much attention is that first propounded by De Lisle, according to which the corona is due to the diffraction of the solar rays passing near the moon's edge. He allowed the sun's light to pass through a small aperture into a dark room, and received the cone of light thus formed on an opaque disc somewhat larger than the cross-section of the cone. When this disc was viewed from behind, a margin of light somewhat resembling the corona could be discerned.

We owe to the late Professor Baden Powell and to Sir David Brewster the thorough investigation of this theory. In the first place, it was pointed out that no experiments resembling those of De Lisle could establish anything respecting the sun's corona, for the simple reason that behind the disc—that is, in the space corresponding to the region beyond the moon—there was air as well as in front—a condition, of course, wholly distinct from that actually prevailing beyond the moon. Researches applied to the actual appearance of rings of light, formed under various conditions, led Sir David Brewster to enquire what relation exists between the apparent magnitude of such artificial coronas and the diameter of the diffracting disc. He found that "in all experiments of the kind the breadth of the ring is totally independent of the magnitude of the diffracting body;" "therefore," as Professor Grant remarks, "the unavoidable conclusion is, that in the case of the natural eclipse the ring would be utterly invisible, on account of the comparatively immense distance of the moon from the earth." He adds, that "this must be considered as a fatal objection to De Lisle's explanation, if the principle upon which it is founded be admitted to be true. But, besides, the diffraction theory is incapable of offering any account whatever of many of the subordinate features of the ring, and therefore, upon this ground alone, it cannot be considered as affording a faithful representation of the phenomenon."

The theory which calls in the aid of the earth's atmosphere presents, as I have said, two distinct forms.

In the first place we have the theory, pure and simple, that the solar rays, by illuminating the upper regions of the air during total eclipse, cause the appearance of the corona. It is undoubtedly in this form that the atmospheric-glare theory was first presented, and to this form of the theory the argu-

ments of those who opposed the view that the corona is a phenomenon of our atmosphere were first directed.*

It is clearly necessary, in order to establish the truth of such a theory, that the path by which the light-rays reach the portion of the air which is supposed to be illuminated should be clearly indicated. It is not sufficient to speak in a general way about the course of light-rays through this or that region, without showing that the light-rays can get there. In this respect the theory is undoubtedly defective, since in place of any evidence showing that the sun's rays traverse the portions of our atmosphere which seem to be illuminated during total eclipse, we have very sufficient evidence that such rays cannot come within many miles of those portions of the air.

In any considerable total eclipse the breadth of the moon's shadow when it falls upon the earth is about 150 miles. So that taking the case of an eclipse occurring when the sun is in the zenith (the most unfavourable case for my argument), it follows that the observer is in the centre of a circular region of the earth 150 miles in diameter, and wholly in shadow (so far, that is, as the sun's direct rays are concerned). For seventy-

* I am careful to point out this fact, because Mr. Lockyer considers that I have misrepresented his opinions, and those of the supporters of the atmospheric-glare theory. As a matter of fact, the first intimation that I had that such a theory had been propounded was from a paper in the monthly notices of the Royal Astronomical Society, by Mr. Baxendell. The next was from a paper by Mr. Lockyer himself, which certainly confirmed my impression that the direct passage of the solar rays through our atmosphere was alone in question. His words were simply, "My conviction has been growing stronger and stronger that the corona is due to the passage of the solar rays through our own atmosphere near the moon's place." There is no word about any possible action by the moon on the rays in question. By "the atmosphere near the moon's place" he clearly signifies (or intends to signify) the atmosphere which lies towards that part of the heavens where the moon is. It will be seen from the text above that others besides Baxendell and myself have interpreted the atmospheric-glare theory as I do; and I may venture to assert that, so far as Mr. Lockyer was concerned, no word of any lunar influence on the rays—of any "possible action near the moon's edge"—was heard of, until a mathematical proof had been put forward of the impossibility of any direct solar rays reaching the part of our atmosphere supposed to be illuminated. The subject was mooted again and again without the moon being called in to aid the theory; and at the last meeting of the Royal Astronomical Society a paper, specially drawn up by Mr. Seacombe at Mr. Lockyer's instance, was read, in which the idea of any lunar action was implicitly negatived; while in the discussion which followed (in which Mr. Lockyer himself took part), no reference was made to the moon's action. If there has been misrepresentation, therefore, it has not been such as I at least am in any way responsible for.

five miles on all sides of him the earth is in shadow. Now we may regard the moon's shadow as rising from this circular base, growing somewhat wider upwards (because at the moon's distance it must have the same diameter as the moon), but not increasing much in width for each hundred miles or so of height. Where does this shadow meet the limits of the earth's atmosphere? We may regard those limits as forming a horizontal plane above the observer; but the question is, "At what height does this plane lie?" It has been thought by some that the upper limits of the atmosphere lie but forty or fifty miles from the sea-level. Others assign a much greater height. Bravais, from a discussion of Lambert's observations of the twilight curve, deduced a height of nearly 100 miles. From observations on the aurora, the height of the atmospheric limits have been set at more than 120 miles. Observations of meteors have resulted in the deduction of a yet more considerable elevation. And lastly, from polariscopic observations, the elevation of the atmospheric limits have been set at more than 200 miles. But let us take a value far beyond any of them, and assign to the atmosphere a height of 1,000 miles; and let us add to this supposition, which surely will be regarded as giving to the atmosphere a sufficient extension,* the suppo-

* Recently Mr. M. Williams, in his "Fuel of the Sun"—a work of considerable interest, and, in many parts, of considerable scientific value—has endeavoured to prove that the atmosphere has no limits; founding his proof on the probability that perfect gases admit of indefinite expansion. It results, according to his views, that every celestial body has an atmosphere proportioned to its own mass, according to a sufficiently simple relation. We know so little of the extent of the atmospheres of planets that I do not care to oppose Mr. Williams's *conclusion*, though I must say that there is no reason, even if his premises were admitted, for believing in this special law of distribution, since it is unquestionable that, if a planet's atmosphere were doubled or halved, there would be no effective forces to take away the excess or supply the deficiency, even on the supposition of that unlimited atmospheric extension and consequent intercommunion imagined by Mr. Williams. But I think it very necessary to point out one important flaw in the reasoning by which he disposes, as he considers, of the ordinarily accepted theory that the atmosphere has limits. It is usually argued that there must be a height where atmospheric expansion is so great that the gravity of the particles of the atmosphere is exactly balanced by the repulsion they exert *inter se*; and it is concluded that above this level there can be no atmosphere. But Mr. Williams remarks that an objection to this reasoning has hitherto escaped notice. If at the limits of the atmosphere the forces were so exactly balanced, the ether would have power to brush off the outer layer of particles, then the next, and so on, until the earth is stripped of its atmosphere altogether. It is always well, when one supposes one has detected some point which nobody else has noticed, to make sure that one is

sition that the air at this height of 1,000 miles is capable of being illuminated by the solar rays in such sort as to seem bright to observers 1,000 miles below. Then the observer may be regarded as placed at the bottom of a circular well of shadow, 150 miles in diameter at the bottom, and about 170 miles in diameter at the top, the walls of this imaginary well being visible to him as light. How large, then, would the black opening at the top—for such it must seem to him—actually appear? A disc of 170 miles at a distance of 1,000 miles subtends an angle of about 9¾ degrees; and such, therefore, would be the size of the disc of darkness. But the moon, which would be at the centre of this black circle, subtends only about half a degree, so that for a breadth of more than 4½ degrees all round the eclipsed moon there would be a ring of darkness; but this is precisely the region where we require to have light, if the corona is to be accounted for by the theory we are now upon.

If it be asked how high the atmosphere must be in order that this ring of darkness may be reduced sufficiently to bring the light close to the moon, the answer is, that the atmosphere must reach right up to the moon's immediate neighbourhood.

Prof. Harkness and Dr. Curtis, in discussing the results of the American eclipse, employ the argument here insisted upon. "The moon's shadow," says the former, "at the point where it enters the earth's atmosphere, usually has a diameter of more than 100 miles; and if it were possible for an observer placed within the shadow to see the illumination of the atmosphere outside of it, the appearance presented would be that of a halo having an inferior diameter much greater than the size of the moon. At the commencement of the totality the moon would be within and tangent to this halo; and as the eclipse

not omitting to notice some objection which everyone else has recognised. Mr. Williams here fails wholly to see that the balancing of forces is not equivalent to their annulment. A body resting on the ground is subject to the balanced forces of its own weight and the earth's resistance; yet it is not, therefore, free to be "brushed off" by the lightest zephyr; nay, it may be able to resist the impulse of a hurricane. And the same is true (on the hypothesis attacked by Mr. Williams) respecting the outermost atoms of the earth's atmosphere. The surrounding ether is as powerless to remove them, as to carry the earth bodily away from the sun.

I am not here advocating the atomic theory, though I confess all other theories of the ultimate constitution of the elements seem inconceivable; I am simply endeavouring to show that this particular objection is founded on a fallacy. The objection founded on the behaviour of gases under a process of rarification, down to the supposed occurrence of a real vacuum, will be worth considering when it has been proved that a real vacuum has been produced. The cessation of the transmission of the electric discharge proves nothing.

progressed, she would move across its interior, until she finally reached its other edge, at which instant the totality would end." Dr. Curtis remarks, that "the notion that the corona may be the luminosity of our own sunlit atmosphere beyond the belt of totality is also both theoretically impossible and practically proven false by the testimony of the photographs. As to the theoretical considerations, it is sufficient to point out that with the diameter of the moon's shadow upon the earth two or three times as great as the vertical extent of our own atmosphere, it is geometrically impossible for an observer near the centre of that shadow to see any portions of our atmosphere which lie *beyond* the cone of darkness—which portions alone, of course, could under the circumstances be illuminated—in apparent contiguity with the moon's limb."

The evidence against the theory derived from this simple consideration,—first pointed out by Mr. Baxendell, I believe,—is so simple and so convincing, that it seems useless to consider further arguments.

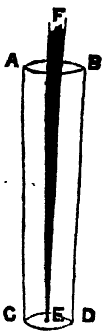
But then there is another mode in which the theory has recently been defended. It is pointed out that it is a mistake altogether to imagine that the advocates of the atmospheric-glare theory had overlooked considerations so simple and so obvious. They had not imagined, it would seem, that the solar rays pass directly into the moon's shadow-cone, but that these rays are introduced into the shadow-cone by means of a possible action exerted near the moon's limb.* The theory is thus made to resemble La Hire's, described in these words by Prof. Grant:—"La Hire suggested that the corona might be produced by the reflection of the solar rays from the inequali-

* Mr. Lockyer remarks, that both Dr. Gould and M. Faye have expressed such an opinion. It is possible that M. Faye may; but I may venture to say, very confidently, that Dr. Gould is not an advocate of the atmospheric-glare theory of the corona, and has advanced no line of reasoning in its support. He has pointed to certain peculiarities observed during the American eclipse (respecting which, however, Dr. Curtis remarks that all other observers are at issue with him); and he remarks respecting these, that they seem to point to parts of the corona as belonging to our atmosphere. But he told me distinctly, after the last meeting of the Royal Astronomical Society, at which he had been present as a visitor, that he had no theory of the corona, and was content with stating what he had seen.

What Dr. Gould has remarked about the moon's possible action is, that the apparent encroachment of the prominence-bases on the lunar disc may be due to specular reflection of the moon's surface. This view has no bearing whatever on the subject of the corona; but if it had, then Dr. Curtis's proof that the encroachment referred to is a purely photographic phenomenon—a view confirmed by Dr. Mayer—would serve to dispose of any reasoning founded on the observed fact.

ties of the moon's surface contiguous to the edge of her disc, combined with their subsequent passage through the terrestrial atmosphere." Prof. Grant, after discussing De Lisle's evidence against La Hire's view, remarks, that clearly "the hypothesis was untenable."

Let us, however, apply what has been already inferred respecting the moon's shadow-cone. Replacing our observer at the bottom of the imagined well of shadow, we have now to consider the case of light making its way into this well through the deflection of the solar rays. But we have one certain fact about the region of shadow. The moon looks black during total eclipse, and therefore it is abundantly evident that lines taken from the eye of the observer to the edge of the moon's limb include within them a cone which is not illuminated. Now we have seen that, taking only the case of undeflected rays, we should have a shadow 170 miles in width at the top and 150 at the bottom, while the cross-section at the top would subtend about $9\frac{3}{4}$ degrees, the moon only subtending about half a degree. Hence the cone of blackness between the eye and the moon's disc occupies a position quite clear of the imagined walls of our shadow-well. The shadow-well, in fact, is shaped somewhat as A B C D in the figure, while the shadow-cone extends from the eye of the observer at E upwards through the middle of the shadow-well, as shown by the black region E F.



Now, before the theory we are dealing with can be accepted, it must be shown how the solar rays, whose direct course would keep them outside A B C D, can be brought within A B C D without trenching at all upon the black cone E F. It is obvious that this is wholly impossible. If we get the rays within A B C D, they can only avoid E F by travelling parallel to its surface (it is to be remembered that they must come quite up to its surface if the corona is to be accounted for); and it would thus follow that the rays which pass the moon's edge are deflected exactly to E. But obviously, though this might happen for a moment during some particular eclipse, it could not by any possibility happen in all eclipses and throughout their continuance, since there is absolutely no fixed relation whatever between the point E and the boundary A B C D of the geometrical shadow-cone. E is simply the station where the observer places himself, and is not necessarily on the axis of the shadow-cone at any moment, and is necessarily away from the axis at all moments save one.

There is, in fact, very little to choose between this form of the theory and the former. It will doubtless be deemed wholly unnecessary to discuss further a theory against which such decisive geometrical arguments may be adduced, especially as, according to its most ardent advocates, the theory has for its main support so vague a conception as that of "a possible action at the moon's surface."

We are reduced, therefore, to accept the sole remaining theory that the corona is a solar appendage; and our subject is proportionately enhanced in interest, since so viewed the corona becomes the most extensive region within the solar system. Granting to it no greater apparent extension than half a degree from the moon's limb, it must yet have a diameter three times as great as the sun's, and therefore a volume (enclosing his globe) twenty-seven times as great. But we may fairly assume that the greatest observed extension of the corona falls far short of its true boundary, since the state of the atmosphere tends importantly to affect the extent of the corona's outline, always reducing it, but not always by the same amount. And since the corona has been observed to extend so far as 8 degrees from the eclipsed moon, or to a distance exceeding some fifteen times the moon's diameter, we should have to assign to the corona a volume exceeding about 30,000 times that of the sun.

But it seems clear that we cannot regard the corona as a solar atmosphere. The arguments founded by Mr. Lockyer on the laboratory experiments of his eminent ally, Professor Frankland, seem to me to be wholly convincing on this point. The pressure of the solar atmosphere at the level of the summits of the prominences must be exceedingly small, since, even near the base of the chromosphere, the pressure is not considerable. From the researches of Wüllner it would seem that, near the base of the chromosphere, the pressure corresponds to a barometric height of between 50 and 500 millimètres—that is, roughly, between two and twenty inches.* Eighty or a hundred thousand miles or so above this level, the pressure

* It seems to me open to doubt whether at the real base of the chromosphere—that is, at the very level where the photosphere and the chromosphere meet—the atmospheric pressure may not exceed, and that enormously (even many thousand fold), the pressure estimated from the observed width of the hydrogen lines at the apparent base of the chromosphere; for the most powerful telescope yet constructed could not recognise as a sensible quantity, a stratum next the photosphere of even 100 miles in thickness. Yet remembering the compound character of the solar atmosphere at its lowest levels, it would be within such a stratum that by far the greatest increase of pressure would take place.

would be inconceivably minute; so that we can scarcely conceive that, above that level, any atmosphere capable of supplying an appreciable amount of light can exist, still less that any atmosphere can extend to the enormous distances at which portions of the corona have been seen to lie above the sun's surface.

It seems to follow, as an inevitable conclusion from this reasoning, that the corona must consist of some sort of matter —discrete solid or liquid bodies, vaporous masses, or groups in which solid or liquid bodies are intermixed with vaporous masses—travelling around the sun. There appears to me absolutely no escape from this conclusion, when we merely consider the evidence against other theories; for I can conceive no other theory of the corona besides this one and those others which we have been forced to reject.

The negative evidence thus bringing us to this particular theory, however, it will be well that we should enquire how far we have positive evidence in its favour.*

In the first place, let me invite attention to what Leverrier has demonstrated respecting the motions of Mercury. The secular motion of the perihelion is not such as it should be if the whole mass of matter within the orbit of Mercury were within the visible boundary of the sun. The conclusion, to which Leverrier has been led by this circumstance, is that a zone or band of small planets exists within the orbit of Mercury. A single considerable planet would produce changes in the motions of Mercury in his orbit, not a change such as is actually observed in the orbit itself. A single planet, then, is not in question, but many. The change, in fact, resembles that produced on the orbit of Mars by the family of asteroidal planets.

Now we cannot neglect such evidence as this, because it is precisely that sort of evidence which has been found most reliable. Unless observation has erred in a systematic and most singular manner, there is a family of small bodies within the orbit of Mercury. What the nature of the family may be Leverrier's researches do not tell us; but, as observations

* In a review of my "Other Worlds" in the *Quarterly Journal of Science* (a review with which I have every reason to feel satisfied, and which has obviously been written by a thoughtful student of science), it is urged that I am too anxious to show that every point of evidence favours a theory of mine—though some points may be urged as well in favour of other theories. I cannot too strongly express my conviction that this anxiety is the very essence of safe theorising. If a theory is clearly opposed by one single point of evidence, it must go overboard, even though a thousand other points seem to favour it.

made during eclipses have revealed no bright points of light,* we may fairly conclude that there are many very small bodies instead of a few bodies of considerable dimensions. Crowds of such small bodies could scarcely fail to be discernible by their combined light during the total obscuration of the sun.

Thus, by an independent line of reasoning, it has been shown that light might be looked for where the corona is actually seen.

But, again, Mr. Baxendell has been led to infer, from observed meteorological and magnetic changes on the earth, that a zone or ring or disc of matter surrounds the sun, extending to a distance closely corresponding to that assigned by Leverrier to the family of planets. The evidence adduced by Baxendell is of a very striking character, and seems almost inexplicable save on the hypothesis he adopts. And clearly, if such a zone or disc of matter exists, we might expect to see it during total eclipse.

Here, then, is a totally distinct reason for expecting that some such object as the corona would appear when the sun is eclipsed.

Then there is the zodiacal light—demonstrably not a terrestrial phenomenon, since it rises and sets with the celestial bodies as the earth rotates. We see this light during the twilight hours; we recognise a gradual condensation towards its core, and still more markedly towards the place of the concealed sun. Is it reasonable to believe that this condensation is suddenly checked before the sun is reached? Or if we assumed this, must we not yet believe that the zodiacal matter forms at least a *zone* around the sun? so that, despite the imagined gap between it and the sun, its light would in appearance reach to the sun's place, did the background of dark sky but reach so far.

Here, then, is a new reason for expecting that during totality light would be seen around the sun.

Then, again, there are the meteor systems, of which the earth encounters more than a hundred, all whose perihelia must necessarily lie within the earth's orbit. The chances are so enormous against the earth's encountering one such system, unless many millions existed, that we are forced to conclude that there are many millions for each system actually encountered by the earth. This is, be it remarked, not a hypothesis, but an inevitable conclusion. These meteor systems, illuminated as they must be by the sun, would be visible where the corona is, even if the perihelia lay as far from the sun as the earth's orbit. But passing, as many must, quite close to him, they must be illuminated yet more brilliantly—probably rendered incandescent, or even vaporised, by intense heat.

* With perhaps one exception during the Ameiican eclipse.

Here, then, is yet another reason for expecting that when the sun is eclipsed light would be seen around him.

The perihelia of comets, again, are known to be very richly distributed in the sun's neighbourhood as compared with more distant regions. Considering that for every discovered comet hundreds escape discovery (a circumstance recognised by all astronomers), and adding to that consideration the discovery that a proportion, at any rate, of known comets have trains of meteoric attendants, we have fresh reason for expecting that the sun's neighbourhood during total obscuration would be a region of brightness.

Lastly, Venus and Mercury, when in inferior conjunction, are seen to be projected as dark bodies on a relatively bright background. The light which illuminates that background would undoubtedly be discernible during the total obscuration of the sun.

Against the theory to which we have thus been led, both by the weight of positive evidence against all other theories, and by the overwhelming weight of evidence in its own favour, no evidence has ever, I believe, been adduced. No one has pretended to point to a single argument which seems opposed to the theory. Nor has any attempt ever been made to support the other theories under the weight of those arguments which have been urged as conclusive against them.

Thus, then, the matter rests at present. I would submit to those who desire to see the cause of astronomy successfully advanced, that a case has been made out for directing future observations to the analysis of the structure and physical condition of a veritable solar appendage, in place of wasting the energy of observers in the attempt to resolve a question which has already been fully answered. If, next December, observers regard the corona as a solar appendage, numberless ways of determining what the nature of that appendage may be can scarcely fail to suggest themselves. Observations made with such an object, and according to methods so suggested, cannot fail to be most instructive and useful. But if, on the other hand, observers regard the corona as an object whose real position in the solar system is undetermined, as an object which may be ninety millions of miles away round the sun, or a quarter of a million of miles away round the moon, or close by within a few hundred miles of us in our own atmosphere, it must needs be that their observations will be ill-directed and relatively valueless. This consideration, and only this, has led me to dwell as strongly as possible, while there is yet time, on considerations which (as I take it) are amply sufficient to guide the thoughtful student of nature to a just general opinion as to the position of the corona in the solar system.

MACHINE-GUNS, OR MITRAILLEUSES.

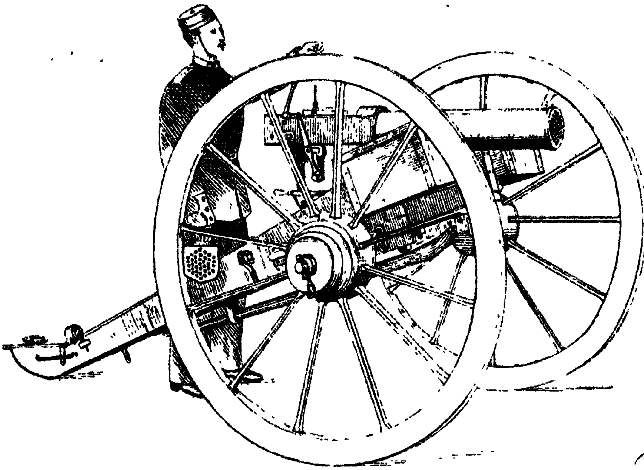
By S. J. MACKIE, C.E.

FOR some time past the public have heard of experiments with machine-guns at the famous government shooting-grounds at Shoeburyness, and the reports of these trials, and the notoriety weapons of this nature have attained in the fearful battles which have recently been waged on the Continent, have combined to give the keenest interest to this subject. The French went into war with much boasting over these terrible mitrailleuses, and these new arms, it is clear, must have made their mark, or we should not now hear of their contemplated introduction into the Prussian armies. The Prussians, like ourselves, have preferred artillery; and, without doubt, if the sole option of one or the other were a necessity, and judgment lay between rifled field-guns and mitrailleuses, the choice of the former for general service would be right and proper. The field-gun can fire buildings, destroy material, and make a breach; whilst the mitraille contained in its shells is carried to long distances with the force of a large charge of gunpowder. Nevertheless, the machine-guns throwing compact clouds of bullets, or streaking with thin horizontal lines the front of the enemy, have special deadly uses, and are, indeed, for certain purposes, most terribly destructive weapons. The war has already done this good—that it has, at least partially, awakened this country to a sense of its military, if not national, insecurity; and the popularisation of some of the prominent items of military mechanical and physical science will serve a very useful purpose at this time, in respect to the discussions which have so freely been awakened.

Of the French mitrailleuses no specimen has yet been brought to trial in this country; but two weapons of the class of machine-guns have undergone an extensive, though not as yet by any means an exhaustive, test. These two are the Montigny-Christophe mitrailleuse and the Gatling machine-gun. The former (fig. 1, Plate 66) is a composite weapon,

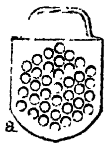
containing numerous—the one under trial thirty-seven—ordinary rifle barrels, with hexagonal exteriors, packed together and soldered into a wrought-iron jacket or cylinder; it is discharged by a descending plate, which allows the separate springs in thirty-seven corresponding chambers to thrust forward their respective pistons to fire the cartridges in front of them. The cartridges are of the class known as "central fire," and very similar to those commonly used in the British service for the Snider rifles of the infantry.

The gun, for the sake of explanation, may be regarded as in three parts: (1) the cylinder of thirty-seven barrels; (2) the breech attachments or breech-box, consisting mainly of two deep vertical side-plates, between which the (3) breech-block slides horizontally to and fro, supported on suitable projections. The breech-box is screwed on to the cylinder of barrels by a ring, and has its rear end closed in by a transverse bar or plate. The breech-block contains the arrangements for firing the cartridges, and when pushed home by the lever closes the whole of the barrels at the rear. In the metal of this breech-block are bored short chambers, corresponding in number and position with the barrels; in each of these is a small steel flanged piston pressed upon by a spiral spring. A plate perforated for the protrusion of the striking points of the pistons covers the inner face of the block. In a space in front of this face the steel plate or shutter slides vertically up and down. In front of this, again, is an outer face-plate, containing as many small steel strikers as there are barrels—in this case, thirty-seven. When the lever is pressed down, the breech-block is forced forwards and closes the gun, and the springs acting on the pistons are put in compression, the pistons then pressing against the vertical sliding-plate or shutter. When the handle of the wheel-gear moving the shutter is worked, the edge of the shutter descends, and as it clears each piston in succession, the latter dart across the vacant space and drive forward the strikers upon the fulminate in the cartridges. The edge of this shutter is bevelled, to ease the action of striking, and is also serrated, or cut into steps, in such a way that no two contiguous cartridges can be fired, the distending effect of the gases diverting the shots when the proximity is too close. When the breech-block is drawn back, a thick steel plate, perforated with thirty-seven holes, into each of which a cartridge is placed, being prevented from falling through by a tight fit and by the flange intended for extraction, is inserted in the opening. When the breech-block is slid forward again the cartridges are pushed into the open rear ends or chambers of the barrels, and the gun is closed up ready for firing. These ammunition plates are replaced as fast

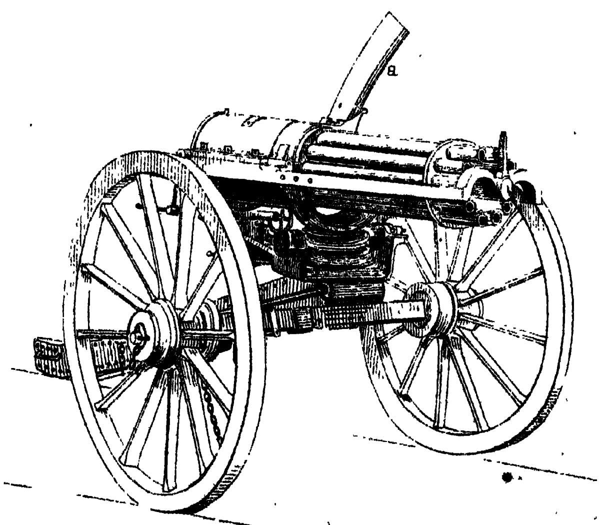


Fosbery-Montigny Mitrailleuse.

a. Cartridge plate



a



Gatling-Gun. a. Cartridge feeding case.

Machine Guns.

W. West lith.

as they are discharged, and the firing proceeds at a high rate of speed. The whole machine-gun is so mounted as to have both vertical and horizontal movements, and the discharge of bullets can therefore be made as a volley by firing all nearly simultaneously by one sudden turn of the handle; or single shots can be separately fired, and by moving the gun horizontally during deliberate firing, the whole front of an enemy could be struck along a line. Major Fosbery considered ten full discharges, or 370 shots, a minute to be within practical attainment; but this speed has not been maintained at the recent trials at Shoeburyness. It is only right, however, to state, that the performance of the Montigny weapon has been largely influenced by defective cartridges. The exact details of the example under trial are—barrels 37; calibre of each 0·534 inch; rifling, Metford's, 5 grooves of 0·011 inch depth, and 0·224 inch width, the intermediate lands being 0·112 inch, the pitch of the rifling one turn in 24 inches (final). The cartridge-charges are 115 grains of gunpowder, and the bullets, made of hardened metal, upon Metford's expanding principle, with hollow of 0·15 inch in base, are each 600 grains in weight. The weapon is temporarily mounted on an ordinary heavy six-pounder field-gun carriage, with axle-boxes ingeniously fitted by Major Fosbery for carrying ten plates ready filled with cartridges; the limber boxes having a reserve of 888, or 24 sets, together 34 volleys. In the waggon 1776 rounds, or 48 volleys. Total, 3,034 rounds, or 82 volleys. The Belgian Montigny mitrailleuses have been made of various sizes, and varying from nineteen to thirty-seven barrels.

The other class of machine-weapons under trial are the "Gatling guns," which differ altogether in principle from the Montigny mitrailleuses, the barrels being fixed apart from each other, but parallel, and the cartridges discharged by a bundle of barrels revolving upon the inclined face of a permanent breech-block, the entire apparatus being most aptly regarded as a huge Colt's revolver. It has as many locks as there are barrels, and all the locks revolve with the barrels. The gun is fed with cartridges through a slot from tin cases containing a specified number, according to the size of the gun. The forward motion of the locks in turning places the cartridges in the rear ends of the barrels, and closes the breech at each discharge, the cartridge shells after firing being extracted in the return movement. The classes of this weapon made at present are, we believe, four. The smallest has ten steel barrels, and is made of any calibre suitable for the musket cartridges used by different nations; the second size has also ten barrels of 0·75-inch calibre, and discharges solid lead bullets of 4½ oz. in weight; another medium size is similarly made of

a calibre of 0·65 inch; and the fourth, or largest size, is made with six barrels usually, but sometimes with ten, of 1-inch calibre, discharging solid lead bullets of half a pound in weight, or a small canister cartridge containing a mitraille of sixteen balls. This weapon also discharges explosive bullets with, it is said, great effect. In the ten-barrelled weapons five cartridges are at the same time in different stages of loading, and five discharged cases in different stages of extraction; or, in other words, the loading, firing, and extracting are automatically performed, the cartridges simply falling out of the ten feed-cases into the slot, and one feed-case after another being supplied as fast as the previous ones are exhausted. As to the speed, the astonishing statements made by Dr. Gatling do not seem to be exaggerations. The large-size weapons can discharge 200 of their half-pound bullets per minute; and from the smaller weapons a fearful fire of nearly double this rate can be kept up, and spread, too, along the line of the enemy's front, the Gatling, like the Montigny, having a horizontal motion on its carriage. The cartridges used have solid drawn metallic cases, and are very effective. As the barrels rapidly revolve, the stream of fire from their muzzles assumes a curvilinear form to the eye, while the drumming noise of the consecutive discharges adds to the fascinating horror of contemplating such murderous instruments.

As to the uses of machine-guns, everybody seems agreed that, for the defence of roads, bridges, streets, defiles, fords, and trenches; in clearing breaches; for flanking faces of fortifications, protecting the decks of ships, and in or against boat operations, they would be invaluable. The other points contested for by the advocates of the new weapons are—the guns are not only very considerably lighter than field-guns, and therefore easy to carry about and to bring rapidly into action, but that, being still too heavy to be affected by the discharges of small quantities of powder, they have no appreciable recoil, and therefore can continue their firing after once being trained, in disregard of smoke or fog, or other obstruction to the vision; and that, if required, they could be placed in position during daylight in order to support or resist a night-attack. As compared with shrapnel shell practice, it is contended that the shell has an iron envelope almost equalling in weight the enclosed bullets, or effective portion of the charge, and that its success depends entirely on the accurate adjustment and performance of the fuze. Thus, in order that the shell should have its full effect, not only must the field-gun be truly laid for distance, and the fuze cut accordingly, but the fuze must act so as to liberate the contents of the shell precisely at the proper moment, when probably two-thirds the weight of metal

put into the gun will produce an effect, the fragments of the iron envelope acting usually very irregularly. Against such perfection of execution it is clear there are frequent and grave obstacles in practice, and in the heat of action, or when firing rapidly at objects in motion, such as infantry and cavalry, whose distances are continually being changed, that some of the requisite conditions must very generally be imperfectly fulfilled. Of the efficiency of machine-guns in comparison with the shell practice of field artillery, we have mainly to consider the conditions of the cones of dispersion formed by the mitraille discharged. From the mitrailleuses and machine-guns these cones will be long, narrow, and the mitraille very uniformly distributed, and the cones of dispersion cut by a target or lines of troops will at every definite distance of range give uniform results. The shells, on the other hand, will have the points of commencement of the cones of dispersion variable, according to the ever-varying positions of the bursts of the shells, whilst the cones themselves will be shorter and wider, and the distribution of the liberated bullets of annular form, or more or less in the shape of a broad ring, in the centre of which will be an almost innocuous area. Such rings would only cut an advancing line of troops in two places; whilst no lateral or streaking motion of the mitraille, seemingly so deadly in effect, is producible in shell practice. The mitraille of the shells, however, covers longitudinally, or from front to rear of the advancing troops, a longer area. The respective weights of guns and mitrailleuses also involves the number of horses and men engaged (the Montigny being about 380 lbs., the Gatling about 250 lbs.); and numerous other items crop up for serious consideration. Indeed, it is only out of the practical results in the present war, and by thorough and exhaustive experimental investigations, calmly and dispassionately carried to logical conclusions, that the real practical general value of machine-guns can be estimated, and the best weapon for the purpose determined.

The following tables give the comparative shootings of the weapons so far put into competition by the committee, directed by the Government, under the presidency of Col. Wray, C.B., R.A., to carry out this investigation. (See pp. 396–406.)

With those accurate returns before them, every one can draw all the comparisons possible at this time; and we conclude this article without comment of any sort, feeling that it would not be proper to pronounce definite views before the conclusion of the trials is announced.

COMPARATIVE EXPERIMENTS OF MONTIGNY MITRAILLEUR AND GATLING GUNS WITH THE FIELD GUNS AND SMALL ARMS OF THE BRITISH SERVICE: NAMELY—

		Barrels	Bullets		Powder in each cartridge
Mitrailleur		37	Each 600 grains (Metford hardened)		115 grs.
Gatling guns—*					
Large	1 inch	10	„ 3,496 „	„	525 „
Medium	·75 „	10	„ 2,100 „	„	300 „
Medium	·65 „	10	„ 1,490 „	„	300 „
Small	·42 „	10	„ 380 „	„	80 „

Field guns firing—		Balls contained in shell	
Shrapnel	9-pr. Muzzle-loading Rifled	63	28 of 18 to the lb. 35 of 34 „
Shrapnel	12-pr. Breech-loading Rifled	56	42 of 18 „ 14 of 34 „
Segment	9-pr. Muzzle-loading Rifled	—	
Segment	12-pr. Breech-loading Rifled	48	Average weight, 1·32 oz.
Case	9-pr. Muzzle-loading Rifled	110	Mixed metal 1 oz. bullets.
Case	12-pr. Breech-loading Rifled	138	Mixed metal 1 oz. bullets.

		Powder
Martini-Henry Breech-loading Rifle	480 grains mixed metal bullet	85 grs.
Snider „ „	480 grains lead „	70 „

* The large and medium Gatling guns are fed by side cases, each containing 15 and 20 rounds respectively. The small gun is fed by a drum containing 368 rounds. The targets fired at were painted with the outlines of cavalry and infantry dummies.

Only shots through and lodged in the targets are counted. The strikes, both from the guns and mitrailleur, were throughout remarkably few in number.

The casualties in files estimated by the number of men and horses struck, no man being allowed to score more than 2 hits in a series, one of which is intended to represent a casualty in the front and the other in the rear rank.

First Experiment.

Firing against time (2 minutes) at a line of targets representing 90 Cavalry or 150 Infantry.

Range	Nature of ordnance or small arms	Projectile	Elevation	Length of fuze	No. of rounds fired	Number of balls lodged and through screen	Cavalry disabled	Infantry disabled	Remarks
yards				in.					
300	Mitrailleur . .	—	0° 34′		*5 / 185	171	60	69	Could have fired another plate, but lost it by a cartridge case being left in bore by a previous round; also some delay from a plate jamming, probably due to broken nipple.
	Small Gatling gun ·42	—	—		616	369	95	116	Mr. Broadwell, Dr. Gatling, and 1 man worked the gun. Trail moved after the first drum. Gun not relaid, and the second drum wasted in consequence.
	9-pr. bronze M. L. R.	Case	1°		10	208	80	79	All broke up well. 11th round in gun.
	12-pr. B. L. R. .	"	1° 15′		9	268	91	111	2 did not break up.
									No. of rounds fired by each of six Infantry soldiers—
	Martini-Henry . .	—	—		142	74	44	36	27, 21, 24, 23, 23, 23 = 141.
	Snider . . .	—	—		83	63	36	29	18, 13, 17, 13, 14, 8 = 83.
400	Mitrailleur . .	—	0° 48′		*6 / 222	178	73	84	1 case left in barrel, consequently 1 cartridge had to be removed each time. Total of miss-fires, 10.
	Small Gatling gun ·42	—	—		456	310	103	144	1 officer and 3 men worked the gun. Traversing by hand. Automatic apparatus not used.
	9-pr. bronze M. L. R.	Case	1° 25′		11	236	77	86	Case shot in axle boxes. Cartridges from rear. 2 miss-fires.
	" "	Shrapnel	0° 25′		7	144*a*	54	66	*a* 10 were from fragments.
	12-pr. B. L. R. .	Case	1° 40′ to 1° 50′		9	166	72	79	1 miss-fire. 2 case shot did not break up. 10th round in gun when time was called.

* Upper figure denotes number of filled plates of cartridges; the lower figure the total of bullets fired.

First Experiment—(*continued*).

Range	Nature of ordnance or small arms	Projectile	Elevation	Length of fuze	No. of rounds fired	Number of balls lodged and through screen	Cavalry disabled	Infantry disabled	Remarks
				in.					*Rounds by each soldier.*
	Martini-Henry	—	—		158	68	31	32	30, 24, 26, 23, 26, 29 = 158.
	Snider	—	—		94	77	43	38	19, 14, 18, 14, 16, 13 = 94.
600	Mitrailleur	—	1° 18′		*6 / 222	127	61	51	3 cases left in bore. 14 cartridges altogether did not go off.
	Small Gatling gun ·42	—	—		657	522	94	151	Mr. Broadwell and 1 man worked the gun. The man at the handle relieved after the first drum. No hitches whatever.
	9-pr. bronze M. L. R.	Shrapnel	0° 35′ to 0° 40′		7	283*h*	68	88	1 through target and burst behind. Boxer 5 seconds mealed powder fuzes used. *h* 12 were from fragments.
	12-pr. B. L. R.	"	1° 0′		7	142	48	58	2 rounds through target and burst behind. 9 seconds fuze.
	Repeated with 5 seconds fuze.	—	1° 3′	0·3	6	127	32	38	21 *fragments. 7th round in gun. Time up.*
	Martini-Henry	—	—		146	52	21	25	29. 23, 25, 23, 22, 24 = 146.
	Snider	—	—		95	63	24	27	18, 14, 18. 16, 15, 14 = 95.
800†	Mitrailleur	—	1° 53′		6 / 196	110	38	45	6 cases stuck. The firing was by volleys. 7th round loaded when time was called.
	"	—	1° 53′		5 / 183	154	59	49	2 cartridges not fired. The firing was by single shots fired rapidly, like "file" firing. 6th round stopped in consequence of a cartridge remaining in barrel.

* Upper figure number of plates; lower of bullets.

† Actually 780 yards.

	Large Gatling gun, 1 inch . . .	—	—		116	7	3		Mr. Broadwell and 6 men worked the gun, which was fed by side cases or feeders. These frequently jammed. All the shots with the exception of 10 passed over the target.
	Small Gatling gun ·42	—	—		357	229	95	114	A jam for 40 seconds about half way through the rounds.
	9-pr. bronze M. L. R.	Shrapnel	1° 5′		5	118	23	27	1 shell burst beyond target. The hollowed-out head of the rammer caught the shells on several occasions and partially withdrew them, thus making the practice very bad. Boxer 5 seconds mealed powder fuze used.
	,, ,, . .	,,	1° 5′		8	115*d*	45	51	A 12-pr. rammer, solid head, used. Boxer 5 seconds mealed powder fuze used. *d* 7 were from fragments.
	12-pr. B. L. R. .	Segment	1° 30′ to 1° 20′		6	38	15	18	2 rounds through target and burst behind. Targets could not be seen distinctly, being obscured by smoke.
	,, ,, . .	Shrapnel	1° 25′	0·3½	6	14	8	5	1 fuze blew. Gun loaded when time was called. Boxer 10 seconds time fuzes used. *This series to be repeated with 5 seconds fuze.*
	Repeated	,,	1° 15′		6	152	56	71	19 *fragments. 7th round ready, but time up.*
	Martini-Henry . .	—	—		136	66	42	31	25, 23, 22, 21, 24, 21 = 136.
	Snider . . .	—	—		102	48	27	17	18, 16, 18, 17, 17, 16 = 102.
1000	Mitrailleur . .	—	2° 34′	0·5	6 } 198 }	33	16	15	Volleys fired by Mr. Christophe. 4 cartridges stuck.
	Small Gatling gun .	—	—		483	62	31	27	Nearly all the shot in this discharge fell from 50 to 60 yards short.
	9-pr. bronze M. L. R.	Shrapnel	1° 50′		8	294	86	110	62 fragments.
	12-pr. B. L. R. .	,,	2° 10′		7	218	69	105	51 fragments. 8th round in gun.
	Martini-Henry .	—	—		124	47	27	30	25, 18, 23, 20, 19, 19 = 124.
	Snider . . .	—	—		—	—	—	—	Considered beyond the range of the Sniders, which are only sighted up to 900 yards.

SECOND EXPERIMENT.

Firing 5 rounds deliberately at a line of targets representing 90 Cavalry or 150 Infantry. The firing of the Martini-Henry and Snider small arms by six men of the Scots Fusilier Guards.

Range	Nature of ordnance or small arms	Projectile	Elevation	Length of fuze	Time		Number of balls lodged and through the screen	Cavalry disabled	Infantry disabled	Remarks
yards				in.	min.	sec.				
300	Mitrailleur . .	5 plates, or 185 rounds	0° 34′		2	55	172	92	83	File firing. 9 miss-fires.
	9-pr. bronze M. L. R.	Case	1° 0′		1	5	162	58	64	
	12-pr. B. L. R. . .	"	1° 0′ to 1° 20′		1	50	138	53	59	6 rounds fired, as one did not break up.
	Martini-Henry . .	129 rounds	—		1	50	83	34	30	*No. of rounds fired by each man*— 23, 21, 19, 19, 24, 23 = 129.
	Snider . . .	90 "	—		1	50	74	44	28	16, 16, 14, 16, 15, 13 = 90.
400	Mitrailleur . .	185 "	0° 48′		3	50	177	83	74	File firing. 9 miss-fires. 9 extra rounds fired to make up for miss-fires. Prior to the practice the barrels were wiped out with a cleaning rod.

	Small Gatling gun	185 rounds	—		1 35	169	58	93	
	9-pr. bronze M.L.R.	Case	1° 25′ to 1° 35′		1 14	110	52	59	
	12-pr. B. L. R.	„	1° 40′ to 1° 50′		Not observed.	118	47	58	7 rounds fired, as 2 did not break up.
									No. of rounds fired by each man—
	Martini-Henry	122 rounds	—		1 50	90	37	29	21, 16, 22, 20, 24, 19=122.
	Snider	84 „	—		—	61	28	26	15, 14, 16, 12, 14, 13=84.
600	Mitrailleur	5 plates, 185 rounds	1° 18′		1 55	107	49	52	4 rounds of file-firing. 1 volley. 4 miss-fires.
	9-pr. bronze M. L. R.	Shrapnel	40° to 45°	0·3 & 0·2½ 0·3	Not taken	78	46	39	16 fragments.
	12-pr. B. L. R.	„	1° 0′			164	43	50	32 fragments.
									No. of rounds fired by each man—
	Martini-Henry	115 rounds	—		1 50	74	39	40	20, 19, 18, 17, 21, 20=115.
	Snider	88 „	—		—	63	34	31	14, 15, 16, 15, 15, 13=88.
800	Mitrailleur	185 „	1° 53′		1 45	106	50	45	4 rounds of file firing. 1 volley. 1 cartridge sheared.
	Small Gatling gun	185 „	—		0 56	183	74	105	
	9-pr. bronze M.L.R.	Shrapnel	1° 5′	0·3½	Not taken	124	51	54	15 fragments.
	12-pr. B. L. R.	„	1° 20′	0·3½	1 35	203	69	77	36 fragments.
									No. of rounds fired by each man—
	Martini-Henry	117 rounds	—		1 50	85	39	55	21, 19, 19, 18, 21, 19=117.
	Snider	83 „	—		1 50	62	32	36	15, 14, 14, 13, 15, 12=83.

For marking—see foot note First Experiment.

Note.—It was not considered necessary to fire the Gatling gun at the intermediate ranges, the officer laying it, and the Committee, being satisfied that nothing would be gained by repeating the practice deliberately with this weapon.

THIRD EXPERIMENT.

Firing against time (2 minutes) at a column of targets representing 90 Infantry, divided into 3 troops or companies, 20 paces apart.

Range	Nature of ordnance or small arms	Projectile	Elevation	Length of fuze	Number of rounds fired	1st screen: Number of balls through and lodged	1st screen: Infantry disabled	2nd screen: Number of balls through and lodged	2nd screen: Infantry disabled	3rd screen: Number of balls through and lodged	3rd screen: Infantry disabled	Total number of balls through and lodged in 3 screens	Total Infantry disabled
yards				in.	Plates								
1200	Mitrailleur		3° 15′		10 / 370	46	21	62	25	93	35	201	81
	Small Gatling gun				326	102	37	78	31	25	16	205	84
	9-pr. bronze M. L. R.	Shrapnel				24	15	24	14	44	28	92	57
	„	Segment				46	13	24	14	29	18	99	45
	12-pr. B. L. R.	Shrapnel				42	20	29	14	7	6	78	40
	„	Segment				90	20	63	25	20	14	173	59
	*Martini-Henry				118	38	16	25	12	21	14	84	42
1400	Mitrailleur		4° 15′		8 / 272	21	8	17	9	30	19	68	36
	Large Gatling gun, 1 inch†		4° 6′		255	27	12	42	22	30	20	99	54
	Small Gatling ·42 „		4° 51′ to 5° 18′		545	24	11	43	21	37	23	104	55
	9-pr. bronze M. L. R.	Shrapnel											
	„	Segment											
	12-pr. B. L. R.	Shrapnel											
	„	Segment											

* *No. of rounds fired by each man*: 21, 19, 21, 19, 19, 19.

† Fired for 1 min. and 18 seconds only, all the ammunition being expended.

Fourth Experiment.

Firing at 134 dummies, placed in loose order on uneven ground, representing broken Infantry retiring.

Left of the line thrown back. Front, 98 yards. Average depth, 35 yards. Firing from three positions at unknown ranges. The distance judged by Officers in command. A different subaltern being appointed for each series.

Guns	Elevation	Length of fuze	Time of Firing	Number of rounds fired	Total disabling hits	Infantry disabled	Remarks
9-pr. M. L. R., firing shrapnel.		in.					
1st position,—judged at 400 yards	0° 20′	0·2	1′ 55″	5	74	30	2 rounds burst *beyond* the line of dummies—ineffective.
fuze altered to	—	0·1½	—				
2nd position,—judged at 700 yards	0° 50′	0·3	3′ 10″	5	34	26	Gimlet borer broken, causing considerable delay.
altered to 750 „	1° 0′	—					
„ 780 „	—	—					
„ 800 „	—	0·3½					
3rd position,—judged at 1,000 „	1° 50′	0·5	2′ 15″	5	47	37	Service hook borer broken, causing delay.
Total of 15 rounds					155	93	
12-pr. B. L. R.,—firing shrapnel.							
1st position—judged at 570 yards	0° 45′	0·2½	2′ 45″	5	29	18	1 round blind, and 1 over—ineffective.
altered to 520 „	—	0·2					
2nd position—judged at 650 „	1° 5′	0·3	2′ 11″	5	82	45	Safety pin broken in the fuze first round, causing delay.
		0·4½					
3rd position—judged at 960 „	2° 0′	0·5	2′ 45″	5	39	33	The changes in length of fuze considerably reduced the rate of firing.
		0·4½					
Total of 15 rounds					150	96	

The mean time firing 5 rounds from each gun, viz., 2½ minutes, was taken as the period for the Infantry and Mitrailleur to fire for comparison.

Fourth Experiment—(*continued*).

GUNS	Elevation	Length of fuze	Time of Firing	Number of rounds fired	Total disabling hits	Infantry disabled	Remarks
Mitrailleur,—firing for 2½ minutes.		in.					
1st position,—judged at 330 yards	0° 36′	—	—	9/333	122	69	1 miss-fire. 1 cartridge case jammed.
altered to 200 „	—	—	—				
2nd position,—judged at 650 „	1° 18′	—	—	9/333	83	59	6 cartridge cases jammed.
3rd position,—judged at 950 „	2° 20′	—	—	11/407	9	8	9 plates were loaded with an improved pattern cartridge, and 2 with the old cartridge hitherto used. 6 of the new cartridges jammed, and 15 of the old ones. The indifferent result is attributed by Mr. Cristophe to the new cartridge being loaded with a much stronger powder, for which due allowance had not been made.
Total of 15 rounds . .					214	136	
Further trial with 12-pr. B. L., firing case-shot. Distance—300 yards	1° 0′	—	2′ 30″	7	83	57	
Small Gatling gun.							
1st position,—300 yards	—	—	2′ 30″	453	312	100	
2nd position,—650 „	—	—	2′ 10″	736	162	92	
3rd position,—950 „	—	—	2′ 5″	736	177	85	
Six Infantry soldiers firing Martini-Henry rifles for 2½ minutes. Lieut. Col. Fletcher in command.							*No. of rounds fired by each man*—
1st position,—judged at 450 yards	—	—	—	148	65	45	22, 25, 24, 24, 27, 26 = 148.
2nd position,—judged at 350 „	—	—	—	133	49	37	18, 22, 24, 20, 25, 24 = 133.
3rd position,—judged at 950 „	—	—	—	110	38	28	17, 20, 19, 17, 18, 19 = 110.
					152	110	
Six Infantry soldiers firing Snider rifles for 2½ minutes. Lieut. Col. Fletcher in command.							
1st position,—judged at 450 yards	—	—	—	—	51	38	18, 17, 18, 18, 18, 20 = 109.
2nd position,—judged at 700 „	—	—	—	—	24	21	18, 17, 17, 17, 20, 19 = 108.
3rd position,—judged at 900 „	—	—	—	—	7	7	14, 15, 14, 16, 20, 17 = 96.
					82	66	

Practice before the Secretary of State for War, and His Royal Highness the Field Marshal Commanding-in-Chief.

Trial of Montigny and Large Gatling Mitrailleurs *v.* Infantry, and 12-pr. B. L., and 9-pr. M. L. R. guns, on the sands, from the old battery. 5 sets of single targets, 6 in each row, representing Infantry and Cavalry at unknown distances, not less than 1000 yards.

Range	Nature of ordnance or small arms	Projectile	Time	Elevation	Length of fuze	Number of rounds fired	Number of balls lodged and through the screen	Cavalry disabled	Infantry disabled	Remarks
Judged at 1100 yards	12-pr. B. L. R.	Shrapnel	Not taken	2° 26′	in. ·55	5	4	2	3	3 fragments. Vent piece blown out at 3rd round. Fuze ordered to be bored shorter, but made longer by mistake.
	9-pr. M. L. R.	"	2′ 48″	2° 15′	·50	5	44	21	20	7 fragments.
	Large Gatling	—	1′ 45″	—	—	270	16	5	8	Practice stopped at 1′ 45″. No more ammunition ready.
	Mitrailleur	—	2′ 40″	3° and 2° 50′	—	10 } 370 }	3	—	2	3 miss-fires. One or two cases stuck. The whole of the cartridges had been greased, and a small slip of paper gummed on to them by Mr. Metford.
	6 Infantry soldiers with Martini-Henry B. L. rifles	—	2′ 48″	—	—	182	17	7	6	33, 30, 30, 27, 30, 32 = 182.

Trial of Montigny and Small Gatling Mitrailleurs v. 12-pr. B. L. and 9-pr. M. L. R. guns, at 800 yards, against 3 rows of 45 feet × 9 feet targets, 15 yards apart, representing columns of Infantry and Cavalry. (Time 2 minutes.)

Range	Nature of ordnance or Small arms	Projectile	Elevation	Number of rounds fired	1st screen: Number of balls through and lodged	1st screen: Cavalry disabled	1st screen: Infantry disabled	2nd screen: Number of balls through and lodged	2nd screen: Cavalry disabled	2nd screen: Infantry disabled	3rd screen: Number of balls through and lodged	3rd screen: Cavalry disabled	3rd screen: Infantry disabled	Total number of balls through and lodged in 3 screens	Total Cavalry disabled	Total Infantry disabled	Remarks
yards 800	12-pr. B. L. R.	Segment with C percussion fuze modified.	1° 20′	6	5	4	1	315	Not recorded	Not recorded	176	42	28	496	Not recorded	Not recorded	Numerous fragments. Seventh round in the gun when time up.
	9-pr. M. L. R.	Shrapnel with screw C percussion fuze modified (Experimental)	1° 5′	7	3	2	3	164	19	26	87	24	30	254*f*	45	59	*f* 19 fragments. 2 rounds premature, and one burst over. Total of effective rounds, four.
	Mitrailleur	—	—	$\left.\frac{11}{407}\right\}$	35	12	10	22	11	8	24	11	3	81	34	21	12th plate in gun. Fired in volleys. Cartridges had been modified and lubricated by Mr. Metford.
	Small Gatling	—	—	82	82	14	25	82	17	30	27	20	17	201	51	72	Dr. Gatling, Mr. Broadwell, 1 assistant, and 3 gunners worked gun. Gun worked stiffly, and the fire checked twice by cartridge jamming.

REVIEWS.

HENFREY'S BOTANY.*

IT is a strange fact, but not the less is it a true one, that while all other branches of science have made immense advances in this and in other countries, Botany has, in England at least, remained pretty nearly as it was ten years ago. Of course we mean to refer to structural and scientific botany, and not to the mere collection and increasing of plants which sometimes we think improperly receives its name; for it indeed is one thing to gather and dry and name a quantity of plants and fruit, and another to discover the general laws by which they obtain their nourishment from the soil, or bring forth the seed in due time, or are distributed over different parts of the globe. We do not for a moment wish it to be inferred that we desire to direct attention to these phenomena above all others. What we do mean and what we will say is this, that it is especially to them that the attention of students should be directed, if any good is to come from regular botanical study. And believing this, it has always been a matter of regret to us that Professor Henfrey was removed from amongst us; for we doubt not that, had he remained, much would now have been done which has been left undone in the department to which we have alluded.

But while we hold this opinion, which it would be unfair not to admit, we must not place ourselves in such a position that we shall not be able to recognise the labours of others in the same field. We must, while we sufficiently regret the dead, not leave ourselves unable to recognise the good and laudable work of the living; and it is for this reason that we should direct attention to recent labourers in the same field, and especially to the active labours of Dr. Masters, now before us.

Before the time of Henfrey, it may well be said that there was no teacher of elementary botany in England. There was, of course, Balfour's work, and Schleiden's admirable treatise, translated by Dr. Lankester; but while the first was a simple, practical summary of the state of knowledge, the latter was a different work, containing very fully the author's able views of some questions like that of fecundation, but very deficient in other par-

* "An Elementary Course of Botany, Structural, Physiological, and Systematic." By Professor Arthur Henfrey, F.R.S. Second edition, revised and in part rewritten by Maxwell T. Masters, M.D., F.R.S. London: Van Voorst, 1870.

ticulars. Then about thirteen years ago came the valuable work, now in its second edition, and from its date came a revolution in English teaching. Perhaps of the few who are working at botany generally, no one was more thoroughly qualified to take up the work and bring it down to the present period than Dr. Masters. And as the work of the preparation of new editions goes on, we must congratulate him upon the clear good sense which influenced him in leaving the great bulk of the volume as it was before. Indeed, with the exception of the portion devoted to the description of the natural orders, we do not observe much change in the work; and for this we beg to offer our best thanks to the learned editor, for he has seen how very far in advance of the mere botanist the former author of the work was, and has recognised it accordingly. It is especially, as far as we can see, in regard to the description of the natural orders, that the author of the new edition has had the part to play, and so far he has done his work well and modestly. Dr. Masters' work has been both laborious and well executed. While he has admitted a great many orders, which, if our memory serves us, were not there before, he has been cautious in doing so, and has not admitted more than the mere necessity of the occasion demanded. Furthermore, he has been terse in all cases,—no mean quality in an editor.

The great bulk of the work is as it was before. In certain cases many additions have been made, in others hardly any. But in all respects we must regard Professor Henfrey's treatise as being the very best text-book on botanical science which our language possesses.

HUMAN HISTOLOGY.*

WITHIN the past five or six years how very little good work has been done in this country in Histology! It is strange that with the best kinds of microscopes, and the cheapest in the world, we have done so little in histology. We cannot understand why so much good work comes from abroad, where microscopes are dearer and worse, as a rule, than ours, unless it be that education is so far ahead of us. But yet why should that prevent large bodies, like the Royal Microscopical Society and the Quekett Club, from undertaking the labour? We cannot tell. But there is no denying the fact that for the one worker at human histology in England there are, on a moderate estimate, from three to five abroad. Even taking the men who have contributed to the present volume alone, we find over thirty who, so far as we can remember, are from that part of the world which rejoices in the name of a fatherland. And where is what we are to show from England?

But if we leave aside for the present the question of comparison, and inquire on what this volume proposes to treat, we find that its subject

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* "Manual of Human and Comparative Histology." Edited by S. Stricker. Vol. I. Translated by Henry Power, M.B., F.R.C.S., Examiner in Physiology in the University of London. The New Sydenham Society. London: 1870.

matter is general, but, so far as possible, confined to man. That is to say, that the different writers treat upon the subject of human histology, and bring in comparative facts whenever there seems a necessity for them. To the medical man especially, the several chapters will be of especial interest, since they bring up the knowledge of the subject to the latest (or nearly so) date. But the first chapter is one which every microscopist would do well to read. It deals with the subject of microscopy, and recounting objects and the different applications of electricity, gas, &c., which have been made of late years by men like Recklinghausen, Stricker, Deville, Kühne, Schultze, and Brücke. It forms a kind of introduction, a preparatory chapter to the work, and is, we believe, from the pen of Herr Stricker himself. The remainder of the work, extending over exactly six hundred pages, deals with cells, connective tissues, nervous tissue, organic muscles, relation between ultimate fibres of nerve, muscles, muscle under the polariscope, the heart, blood-vessels, lymphatics, spleen, thyrus, thyroid, blood, salivary glands, teeth, the whole alimentary and intestinal canal, and, lastly, of the blood-vessels connected with the alimentary canal. In each case the most recent views are given, and the volume is full of interest to those to whom it has any interest. We cannot speak too well of Mr. Power's efforts as a translator. He has had a difficult task to perform, and he has done it admirably well.

ON HEAT.*

WE see, with not a little satisfaction, that this work has actually gone through four separate editions since the year 1863, and so far we have every reason to be satisfied. It must be confessed that it is the only work which the English student finds convenient. But this is not all; it is carefully brought up in each edition to the state of actual knowledge of the time, and even the present edition contains matters and facts not to be found in any of the earlier issues.

It will be well, therefore, if we pay attention to some of the more striking of the novelties which the present volume possesses over its predecessors. Firstly, we have in this the relations of gaseous matter to the shorter waves of the spectrum. Then comes the consideration of the blue of the sky and the polarisation of its light; and, lastly, we have the hypothesis regarding the constitution of comets, or the polarisation of heat. Perhaps of all these the sky-question is at once the most generally interesting and practicable of all. We shall endeavour to follow the professor's description, though it is a little difficult, because of the space at our disposal to do so. Having described fully a small tube filled partly with nitrate of amyl, and peculiarly connected, he goes on as follows: "Opening the cock cautiously, the air of the room passes in the first place through

* "Heat a Mode of Motion." By John Tyndall, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain. Fourth edition. London: Longmans, 1870.

the cotton-wool which holds back the numberless organic germs and dust-particles floating in the atmosphere. The air thus cleansed passes into U-shaped vessel, where it is dried by the sulphuric acid. It then descends through the narrow tube to the bottom of the little flask, and escapes there through a small orifice into the liquid. Through this it bubbles, loading itself to some extent with nitrite of amyl vapour, and then the air and vapour enter the experimental tube together. We will now permit the electric beam to play upon this invisible vapour. The lens of the lamp is so situated as to render the beam convergent, the focus being formed about the middle of the tube. You will notice that the space remains dark for a moment after the turning on of the beam; but the chemical action will be so rapid that attention is requisite to mark this interval of darkness. I ignite the lamp; the tube for a moment seems empty; but a luminous white cloud immediately fills the beam. It has in fact shaken asunder the molecules of the nitrite of amyl, and brought down upon itself a shower of particles, which cause it to flash forth in your presence like a solid luminous spear. This experiment, moreover, illustrates the fact that however intense a beam of light may be, it remains invisible unless it has something to shine upon. Space, though traversed by the rays from all suns and all stars, is itself unseen. Not even the ether, which fills space, and whose motions are the light of the universe, is itself visible."

We take the foregoing, as one of the most striking of the novelties in this admirable work, and also one of the most interesting of the recent facts recorded by the author. But the whole work is full of such, and we cannot do better than recommend those of our readers who are already unfamiliar with it, to procure it for themselves at once. It is not only the best work on the subject in the English language, but it is in itself especially valuable as an eloquent and comprehensive treatise.

BUILDING.*

THE author tells us of this work, that he has endeavoured to introduce the student of architecture to a general outline of scientific subjects connected with his profession, an acquaintance with which at present involves the reading of a large amount of works by various authorities. By so doing, he hopes to command the attention of the great mass of architects, for the mathematical knowledge required to understand what he has got to disclose, does not extend beyond elementary geometry and algebra. The book is divided into seven chapters, dealing with the following subjects: mechanical principles, retaining walls, arches and cupolas; building stones, timber, iron, and lastly water contained in vessels and pipes. So far as we have examined, the book seems clear and intelligibly arranged; but we must differ from the author, when he says, as he does say, that it is the only work of its kind. Unfortunately, too many books of all kinds are pub-

* "The Science of Building. An Elementary Treatise on the Principles of Construction, especially adapted to the requirements of Architectural Students." By E. Wyndham Tarn, M.A., Lond. London: Lockwood, 1870.

lished, and the present one is not an exception to the rule, though we are bound to say of it, that it is, so far as we have seen, one of the good ones. Not the least practical nor useful parts of the book are the last few pages, in which the specific gravity and weight of a cubic foot of nearly all building materials are given.

SMITHSONIAN REPORT.*

IT is a pity, we think, that this excellent volume is not by some means or other published at an earlier date. The volume now before us, is that for 1868, and yet it has only just reached us, and it has only been published, according to the statement on the title-page, in 1869. Now, either one of two things; either it has not been issued at the date mentioned on its page, or if so, it has not been sent out for review. We mention this, because it is an unusual thing in connection with American books, unless those of associations, and we should gladly receive an answer from those in authority.

The present volume is an instructive one, and is in every respect like those which have preceded it. It is a somewhat singular fact that these "Smithsonian Reports" differ from everything else in the shape of reports published. Yet they are a very valuable series which might, we think, be made better under some of the circumstances. For example, we think—though of course the subject is open to many opinions—that there are too many translations from different Continental journals, and that original memoirs might take the place of these translations with the greatest advantage. For though they may give a greater and better tone to the studies of many, they are insufficient. What we mean is, that they are unequal to the demands of most scientific men, besides being behind the time, and that evidently it was not the original intention of the founder that they should appear so fully as they do. We do not mean for a moment to say that the volume is not most valuable, for it is so, and unequivocally so. But we find fault with the editors for receiving a number of translations of papers which, however interesting they may be to the general reader, are of little value to the scientific man. Half of the present volume is filled with the secretary's report, the list of meteorological observers, and the report of proceedings of the Board of Regents. Then comes the general report, which consists of a number of essays, some of them extremely old, and translated or borrowed from the various scientific journals. The first is a Memoir of Cuvier, by M. Flourens, which originally appeared in 1834, and can hardly have any scientific value at the present day. Then follows a History of Cuvier's Works by the same author. Then comes a Memoir of Oersted, by M. E. De Beaumont; one of Schœnbein; one of Encke; one of Eaton Hodgkinson, of University College, London; one on the Recent Papers in Relation to Heat, by M. A. Cazin; one on the Principles of the Mechanical Theory of Heat, by Dr. John Müller (this is valuable); one on the Vibratory Movements of all Matter,

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* "Annual Report of the Board of Regents of the Smithsonian Institution, showing the Operations, Expenditure, and Condition of the Institution, for the year 1868." Washington Government Printing Office, 1869.

by L. Magrini; Professor Tyndall's Rede Lecture on Radiation; on Meteorites, by M. Daubier; on Electric Resonance of Mountains, by H. de Saussure; and several others which we cannot now mention. They all contain interesting valuable matter, and are all of interest; but we think it questionable whether the proper object of the society is fulfilled in reproducing them.

A STAR-ATLAS.*

THIS work, which barely "came in for" an announcement in our last number, is now before us, and we must say a word or two in its favour. Firstly of the contents of the work. It shows all the stars visible to the naked eye in its twelve circular maps, and has in addition two "Index plates" in which the six northern and six southern maps are exhibited in their proper relative positions. Both of them are capitally executed. The two maps of the northern and southern hemispheres are not so perfect as we could wish, but this is no fault of the author's, but rather due to the circumstance that an immense multitude of things had to be brought within a limited space. But when we consider that they are merely general, and that their object is merely to show the student generally how the stars are arranged as a whole around the globe, they are very complete. It is when we come to consider the twelve maps, six of the northern hemisphere and six of the southern, that we are compelled to offer our best thanks to Mr. Proctor for the great labour and time which he must have spent in his endeavour to bring them up to their present perfection. They are very fine, being each about eighteen inches in diameter, and are on a sort of grey outline, and contain to the seventh magnitude of the stars (the different magnitudes being well arranged), and finally the nebulæ. Though we cannot afford space to follow the author through the lengthened argument he adopts to show that the method adopted of placing the stars is the best, we must say that for ourselves we do not see any better plan; and though Mr. Proctor's is not perfection, it is so much better than any of the plans hitherto devised, that we cannot but congratulate him on its selection. We think the author has done well to arrange the stars according to the plan adopted in the Catalogue of the British Association, and it seems to us that he is if anything too apologetic to his readers. His work is well done, and must have involved immense labour. Take the following as an instance of the kind of work the author has had to go through. "It must be remembered that, owing to the great size of the originals, the work of mapping was very wearying. The map had to be placed horizontally because of the large catalogue I had to refer to, and also because a star near the top of a map might be followed by a star near the bottom, &c., so that no slope could be adopted which would have been convenient for marking in successive stars. Thus all stars falling near the upper part of a map necessitated an altitude by no means favourable for good writing.

* "A Star-Atlas for the Library, the School, and the Observatory," &c. Drawn by Richard A. Proctor, B.A., F.R.A.S., and photo-lithographed by L. Brothers, F.R.A.S. London: Longmans, 1870.

Then, owing to the size of the maps, a stout kind of paper had to be used, in which steel pens and compasses worked (at least in my unpractised hands) most unpleasingly." But for all this the author has produced a set of maps of the Heavens which are as excellent in mere details of printing as they are accurate and reliable in an astronomical sense.

HYDROSTATICS AND SOUND.*

THE author of this book tells us that it contains all that is required for the London University, and we are willing to believe him. At all events we can see that it is as much before other books of the kind as the London University examinations are in advance of most others. Assuredly, if this little work contains all that a student must know in respect to hydrostatics and sound, no very severe test exists at Burlington House; for it must be confessed that it deals with only the elements of the two subjects, and that there is within—including questions—a space of about 140 pp. of a duodecimo volume. But after all this does not appear so strange, if the reader will but remember that the book is written by one who is himself familiar enough with the London University examinations. He has cut from his pages all those useless accounts of appliances and valueless descriptions which so many of the old manuals are filled with. He has thus given himself room for the introduction of the really useful matter which fills his pages. In the manual he deals with the subjects of Hydrostatics and Sound clearly and concisely; and as each chapter is followed by a table of questions to which the answers are given, and as the mode of replying to them is sufficiently explained in the text, we see no reason why the student should experience any difficulty in working through its pages. Its general "get up" is excellent, whether in regard to the type in which it is printed, or the excellent diagrams and figures which it contains.

THE CHEMISTRY OF CREATION.†

THE little work which is now before us was written some years ago and was not published. This is a statement made in the preface by the author. We can only say in passing that we wish it had never been published: 1stly, because it cannot convince the disciples of Gliddon; and 2ndly, because, not doing so, it is obviously of no use; unless, perhaps, it may do in America what it would not possibly do here—it may bring in something to the author. Such works are ineffective; for, while they all start with some point or argument which would be denied to the author by his op-

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* "An Elementary Course of Hydrostatics and Sound," &c. By Richard Wormell, M.A., B.Sc. London: Groombridge. 1870.

† "The Chemical History of the Six Days of Creation." By John Phin, C.E., editor of the "Technologist." New York: American News Company. 1870.

ponent, they all give a somewhat different account of the earth. In some the whole of creation was formed—with that which is now fossil and that which we see before us—in the six days. Others tell us that the days may be regarded as representing ages, differing of course in duration. A third will not recognise geology at all, but stick to the original. So it is with the Biblical authorities. Very few of those who differ from them take the least trouble in the matter. Nor can we see that they are wrong in so doing. It in no way affects Christianity what way we look at the commencement of the world in, and we can only hope that those who could well reply to works like the present will stay their hands, and not keep up the "shindy" which the authors so much desire. We are speaking now of the class of works to which the present one belongs, and not of itself. In tone it is not ungenerous, while in style it is superior to the great mass of such works, and we doubt not the author is fully persuaded of the accuracy and intelligence with which he has laid down his views. We do not contradict him, but we think he would do better in any other line, and we recommend him to make the attempt.

THE LABORATORY GUIDE.*

WE need only shortly notice this, as it is a second edition of a work by a writer now well known to chemical readers. It is especially a work intended for agricultural students, and so we cannot criticise it as if it were intended for general readers. Were it otherwise, our comments would be more severe than they shall be. Intended as it is for agricultural people, it is not of so much consequence. It is a curious book, and we have been unable to follow it out. It seems to run through the subject differently to what other books do, and we find ourselves taken about by it from one point to another in a way that we don't exactly understand. However this may do for the agricultural student, we ourselves don't like it much, and we confess that we prefer the ordinary methods of teaching. With these faults the book is a good one, and shows that Professor Church is attentive to the changes in his science.

SPON'S ENGINEER'S TABLES.†

THIS useful little book should have been noticed in an earlier number, but by some accident it was mislaid. A notice of it now will not, we hope, be unavailing, even though it is late. It is almost out of our power to give a full account of the contents, for, though the book is of the smallest size, it contains a multitude of things, though very little of each. But when we state that it contains the following memoranda, we shall have done

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* "The Laboratory Guide; a Manual of Practical Chemistry for Colleges and Schools, especially arranged for Agricultural Students." By A. H. Church, M.A. 2nd edition. London: Van Voorst. 1870.

† Spon's "Tables and Memoranda for Engineers," selected and arranged by J. T. Hurst. London: E. & F. Spon, 1870.

almost all that is required of us. First of all there come excavators' memoranda, then bricklayers', then masons', slaters', carpenters', plasterers', smiths' and founders', plumbers', and lastly, painters' and glaziers' memoranda. Then follow sundries, involving the weight per cubic foot of metals, earth, timber, &c., &c., next mensuration, then money tables, and lastly, a table of weights and measures. With all these, it must prove very valuable to the practical man engaged in almost any trade, while its small size renders it available for the waistcoat pocket.

OUR GARDENS.*

BOOKS written upon this subject are generally such abominable trash, that one doesn't care even to read their titles. In fact, in England gardening has so completely passed into the unworthy and ignorant hands of the mere gardener, that it is almost hopeless to expect an eradication. However, if anything can do so, we expect that the publication of works on gardening like the present will effect it. Now, the myriads of gardens which exist in this country, and especially those kept by persons in London, are under the control of persons who are so ignorant, that they hardly deserve to be called gardeners at all. We can only hope that a better time approaches, and in this hope we venture to commend the work which is now before us, as one unusual of its kind, as it is also original in design. Mr. Robinson has our best wishes in his effort to recall the ancient system of English gardening. We hope his efforts may meet with the success they so well deserve; but we fear that the present system is one which, thoroughly expensive as it is, will yet hold out for long against the storm that may, and necessarily must, be raised against it.

However, be that as it may, the author of this work shows us how we may set about a revolution. He gives us a list of plants that will not be carried off by our winter-frosts, and which extends over eighty pages of his interesting manual. It is almost absurd to think that the myriads of plants of which our gardens are possessed, will not hold out against even the winters of this country. It is absurd to allow such plants to have the larger share. Let them have a share, by all means, for we should have all kinds; but let us give a preference to our own plants, or those that will grow with us, and group them as advised by Mr. Robinson, and let us see whether we shall not do well. Assuredly, at all events, we shall not seek in vain, as it has hitherto been attempted before.

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* "The Wild Garden, or our Groves and Shrubberies made beautiful by the Naturalisation of Hardy Exotic Plants, with a chapter on the Garden of British Wild Flowers." By W. Robinson. London: Murray, 1870.

ELECTRICITY.*

PROFESSOR TYNDALL has here given us the notes of his Lectures on Electrical Phenomena and Theories, and we thank him for doing so. They are a very clear and remarkably concise account of the whole subject, from the earliest experiments to the latest period, and are yet extremely and remarkably clear. It would be impossible to give an abstract of a book which is in itself abstractive, but a reference to the chapter entitled "Historic Jottings concerning the Electric Telegraph" will enable us to give an idea of the nature of the book. After Nicholson and Carlisle had discovered the decomposition of water by the galvanic current, Sömmering and Professor Coxe about the same time proposed a system of telegraphy based on the discovery. In 1820 Oersted discovered the deflection of the magnetic needle. The relation of electricity to the magnetic needle was then worked at by several. It was worked at by Professor Ritchie of the Royal Institution, and in 1832 Baron Schilling constructed models of an apparatus, which were exhibited before the Emperors Alexander and Nicholas. The next step seems to us to have been taken by Steinheil, who in 1837 had established a system of wires about 40,000 feet long, connecting various parts of the city of Munich; and he discovered that the earth alone answers the purpose of the second wire, which need not therefore be employed. From this stage the progress of the invention is obvious. Cooke and Wheatstone entered into partnership, and from 1837 succeeded in introducing the telegraphic system into England, from which time, of course, many improvements have been made. We have, however, taken sufficient from this little book to show its value, and we now close our notice with the hope that our readers will think as we do about the matter.

* "Notes of a Course of Seven Lectures on Electrical Phenomena and Theories," delivered at the Royal Institution of Great Britain, April 28 to June 9, 1870. By John Tyndall, F.R.S. London: Longmans, 1870.

SCIENTIFIC SUMMARY.

ASTRONOMY.

The Eclipse of December 22.—Mr. Hind has published a Nautical Almanac Circular with reference to the path of the total phase in regions which are conveniently accessible. After crossing the Atlantic Ocean, the shadow of the moon passes across the south of Portugal and the Straits of Gibraltar to Algeria, reaching its most southerly limits in about longitude 4° east of Greenwich, where the southern boundary of the shadow-path is in about 34¾° north latitude. Thence the shadow passes to Sicily, the northern limit passing slightly to the north of Mount Etna, and so, touching the extreme southern point of the Italian peninsula, by the south of Turkey, past Thessaly. The most important parts of the shadow's path are those across the south of Portugal and Spain, in Algeria, and across Sicily. The chief towns which lie close to the central line are Odemira, Silves, Almodovar, Tavira, Ayamonte, Huelva, Palos, Xeres, Cadiz, San Fernando, Arcos, Estepona, and Marbella, in the Spanish peninsula; Oran and Ratna in Africa, and Syracuse in Sicily.

Incredible as it may seem, the Admiralty have refused the use of a ship to convey the astronomers and others who had volunteered to observe the eclipse. It will be felt by all that the chief misfortune here is not the loss of the opportunity of making observations likely to throw light on the subject of solar physics. Important as the results of well-concerted observations would in all probability have been, we could better have sacrificed the results of the observation of fifty total eclipses than have suffered the shame of such paltry parsimony to rest for ever as a blot on the scientific repute of England. To all ages the story will be handed down that in 1870 England seemed to have forgotten the traditions of past munificence in the cause of science, and that when sixty generous men volunteered their services, at no unimportant sacrifice, England could not find a corner for them in all her navy. Where George III. could be generous, insomuch that under his auspices ships were sent to the Antipodes to observe a transit of Venus, a Liberal government in this nineteenth century can risk the reputation of the country through a miserly economy.

But then there is war, and who can say what risks this country might not run if any one of her ships should be employed to leave observing parties at Gibraltar or Syracuse, and to transport them back again to England? The prospect is terrible indeed. Yet unless we err, this country has done more

than that in the cause of science when Europe was in arms from the North Cape to Gibraltar and Matapan. France also, when in the throes of a terrible revolution, and threatened by armies on all sides, could employ Delambre and Méchain to measure an arc of the meridian, under circumstances of such difficulty and danger that the completion of the task is among the wonders of science. Truly our boasted progress is of questionable value, if the spirit which has rendered it possible is to be thus forgotten.

The Sun's Corona.—The subject of the sun's corona has attracted a considerable share of attention among astronomers of late. The progress of discussion thus far has not been without effect, Mr. Lockyer having withdrawn his support from the theory that the corona is due simply to the passage of the direct solar rays through our atmosphere. He has now reverted to La Hire's theory that a possible action at the moon's limb may be in question. Mr. Seabroke has drawn up a defence of the original form of the theory in a paper on "The Determination whether the Corona is a Solar or Terrestrial Phenomenon." He takes the case of a point on the earth "where the limbs of the sun and moon are in line; that is, where the eclipse is total, exactly," and proves in a sufficiently satisfactory manner that under these circumstances the spectrum seen by Col. Tennant might be accounted for, as also the different spectra seen by the American observers. Only one thing seems needed to make the demonstration complete; namely, that the Indian and American eclipses should have been "total, exactly." As they were in reality very far from being only exactly total, and as all the conditions of the problem are thus completely modified, it would scarcely seem that Mr. Seabroke's demonstration is to be regarded as complete. It may be remarked, however, that even if it were, it would only demonstrate the possibility that the spectroscopic observations can be accounted for in one particular way, by no means that they cannot be accounted for in any other way. As a matter of fact, it seems abundantly clear that the spectroscopic evidence, like all the direct evidence hitherto adduced, flatly contradicts the theory that the corona is a terrestrial phenomenon; but even if it did not, it could not establish that theory.

Orbit of α Centauri.—The star α Centauri is perhaps the most remarkable double star in the heavens. The apparent path in which the companion moves is very nearly a straight line, owing to the plane of the orbit being nearly directed towards us. The determination of the real path pursued by the smaller star around the larger is not therefore rendered impossible, but the problem becomes exceedingly difficult. The results obtained hitherto have not shown a very satisfactory agreement. Thus Captain Jacob makes the longitude of the ascending node of the orbit 86° 7′, the distance of the perihelion from the node 291° 22′; the inclination 47° 56′, and the period 77 years. Hind makes these quantities respectively 17° 22′, 26° 2′, 62° 53′, and 81·40 years. Mr. E. B. Powell had made the longitude of the ascending node 177° 50′, the inclination 77° 50′, and the period 75·30 years. He has lately obtained fresh data for solving the problem. He remarks that "he had invited attention in 1864 to the important part of the orbit about to be described, viz. the extremity of the perspective ellipse corresponding to the lesser maximum of distance." "I have now to state," he proceeds, "that

the companion has, so to speak, doubled the above extremity, and that consequently the orbit can be determined very approximately. Hitherto it was impossible, as the lamented Captain Jacobs remarked, to say how far the apparent ellipse extended in a northerly direction, and correspondence between observation and calculation did not suffice to establish the correctness of a set of elements. Now, however, though even four or five additional years will enable us to improve the orbit, especially as to the time of periastral passage, the results I have arrived at undoubtedly approximate pretty closely to the truth."

These results are, for the perspective orbit—

Semiaxis major	17″	Position angle for distance	18° 45′
Semiaxis minor	2″·8	Greater minimum „	3″·98
Greater maximum distance	23″·8	Position angle for „	301° 45′
Position angle for „	210° 40′	Lesser minimum „	1″·16
Lesser maximum „	10″·4	Position angle for „	115° 30′

These values result in the following elements for the real orbit:—

Longitude of the periastre	38° 40′	Mean distance	20″·13
Eccentricity	·63944	Period	76·25 years
Longitude of Rising Node	24° 18′	Epoch of periastral passage	1874·2
Inclination	81° 13′		

Spectrum of a Solar Prominence.—Professor Young, of America, has made a remarkable observation. On April 9, 1870, there was an exceedingly bright prominence on the south-west limb of the sun, near, but not over a large spot which was passing off. At the base of this prominence, which was shaped like a double ostrich plume, the C line was intensely brilliant, so that the slit could be opened to its whole width in studying the form of the prominence, but this line was not in the least distorted. On the other hand the F line, also very brilliant, was shattered all to pieces, so that at its base it was three or four times as wide as it ordinarily is, and several portions were entirely detached from the rest. This is a most perplexing result, and seems to throw doubt on the interpretation which has hitherto been given to the displacement of the solar spectral lines. As Professor Young remarks, "Since the C line was not similarly affected, it is hardly possible to attribute this breaking up of F to cyclonic motions in the gas from which the light emanates, and it becomes very difficult to imagine a cause that can thus disturb a single line of the spectrum itself." "Possibly," he adds, but we must admit we can hardly conceive the possibility, "the appearance may be the result of local absorptions acting upon a line greatly widened by increase of pressure or temperature." In other words, as we understand him, Professor Young would imply that the bright F line was really undistorted, though widened, while distorted absorption lines belonging to some other element produced the appearance of shattering. But apart from the difficulty of assuming cyclonic motions in this other element, around a relatively quiescent hydrogen-core, we know of no elements having lines close by F strong enough to produce the observed result. The apparent dissociation of the F and C lines is a phenomenon of a very perplexing character.

The Physical Condition of the Sun.—Dr. Zöllner, whose pictures of solar prominences will be known to many of our readers, has written an interest-

ing paper on the temperature and physical condition of the sun. Assuming that those prominences which present the appearance of eruptions are really produced by the action of explosive forces, projecting vast quantities of glowing hydrogen into the chromosphere, he applies the principles of thermodynamics to determine the heat and pressure at different parts of the sun's mass and atmosphere. He obtains as a probable minimum value for the temperature of the chromosphere 27,700° Centigrade, and for the temperature in the interior region whence the hydrogen is erupted 68,400° Centigrade. Assuming the atmospheric pressure at the base of the chromosphere to be 0·180 mm. (about 7 inches of the mercurial barometer), he finds the pressure at the level of the nuclei of the spots to be about 184,000 atmospheres, and the pressure in the inner region before named no less than 4,070,000 atmospheres. He further deduces the following general results :—

1. We cannot conclude from the want of certain lines in the spectrum of a self-luminous star that the corresponding elements are necessarily absent from its substance.

2. The layer in which the reversal of the spectrum takes place is different for each substance, and lies nearer to the centre of the star according as the vapour-density is greater and as the emissive power is less.

3. Under otherwise equal circumstances this stratum lies nearer to the centre as the intensity of gravity is greater.

4. The distances separating the reversal strata of given substances from each other, as well as from the centre, are greater as the temperature is greater.

5. Under otherwise equal circumstances the spectra of different stars are richer in lines the lower the temperature, and the greater the mass of the star.

6. The difference in the intensity of different dark lines in the spectrum of the sun and other stars does not depend only on the difference between the absorptive powers of the corresponding elements, but also on the different depths at which the reversal of the spectra in question takes place.

Is the Resolvability of Star-groups a test of Distance?—It has hitherto been regarded as tolerably certain that the power necessary to effect the resolution of star-groups (including stellar nebulæ) affords a satisfactory general means of estimating the relative distance of such groups. Mr. Proctor considers that he has been able to prove that this test is altogether untrustworthy. It would be sufficient, if we had evidence of a general uniformity of texture, so to speak, in star-systems; but the evidence we have is opposed altogether, he considers, to such a view. He quotes Sir John Herschel's evidence respecting the Magellanic clouds, as tending to prove that portions of a star-system which lie at nearly equal distances may present wholly different characteristics as respects resolvability. Thus, Sir John Herschel says in one place, in recording his observations of the Nubecula Minor, "We are now in *the cloud*, the field begins to be full of a faint light, perfectly irresolvable." In another place he notes, "Upper limit, *but here it is starry, at the other limit nebulous.*" Elsewhere again, "The main body *is* resolved, but barely. . . . The borders fade away insensibly, and are less, or not at all, resolved." Yet the relative distances of these portions cannot be very unequal.

The Colour of Jupiter.—Jupiter has now returned to our skies, and will for several months be favourably situated for observation, coming to opposition on Dec. 13. Mr. Browning, referring to a note in the "Report of the Astronomer Royal to the Board of Visitors," which tends to throw doubt on the idea of any change in the colour of Jupiter's belts, remarks that as the note compares the present colours of the belts with the colours observed eight or nine years ago, it may not throw any light on the question. "There is some reason to believe that the change of colour in the equatorial belt of Jupiter is periodical." "That this belt," he adds, "was differently coloured during the presentation of 1868–9" (than during the last presentation), "is a fact attested by some six or seven, at least, well-known and skilful observers. It is true that in every case these observers were using reflectors (of apertures varying between 6 and 12 inches), but I have heard from observers who have used achromatics varying between 4 and 8 inches in aperture, that they distinctly remarked the change in colour, although they do not seem to have seen it so plainly as those who have used reflectors. Unfortunately none of these observers have published the results of their observations." Since that was written, Mr. Browning has received a paper from Dr. Mayer, of Pennsylvania, describing observations of the colours of Jupiter's belts during the presentation of 1870, which accord perfectly with Mr. Browning's.

The Eclipse of December 22, as partially visible in England.—The following are the chief elements of the eclipse for different places in the British Isles, the hours given being for mean time at the respective places :—

	Begins	Greatest Phase	Ends	Magnitude
	h. m.	h. m.	h. m.	
Greenwich .	11 7·7 A.M.	0 25·1 P.M.	1 42·1 P.M.	0·814
Cambridge .	11 8·6 "	0 25·5 "	1 41·9 "	0·808
Oxford .	11 0·8 "	0 18·0 "	1 35·0 "	0·813
Liverpool .	10 52·2 "	0 8·0 "	1 24·0 "	0·804
Edinburgh .	10 52·8 "	0 6·7 "	1 20·8 "	0·788
Dublin .	10 34·1 "	11 49·6 A.M.	1 5·9 "	0·812

The Planet Mars.—Mars, towards the close of the quarter, will be favourably situated for observation in the morning hours; but he does not come to opposition until March next.

The November Meteors.—The members of this shooting-star system should be carefully studied, although it is not likely that there will be any remarkable display this year. If observations could be made with a Herschel-Browning spectroscope, we might be enabled to judge whether the meteors which lie at a considerable distance from the cometic nucleus of the system differ in constitution appreciably from those which made their appearance in 1866, shortly after that nucleus (Tempel's comet) passed its perihelion.

BOTANY.

Variation of Species.—In reference to *Bidens cernua.* While collecting along the alluvial, marshy borders of the Potomac below Alexandria, some years ago, Mr. A. H. Curtiss found this species (not before discovered so far south) growing to the extraordinary height of five feet. This, compared with Gray's maximum height, will be seen to be in the ratio of six to one; while in the instance of *B. chrysanthemoides*, it was only three and a half to one. The press would barely admit of smaller branches, while in collecting the same species in New York, we have easily pressed two entire plants side by side. As if this were not a sufficiently surprising effort of nature, on proceeding some distance farther, we came upon some plants of *Oxalis stricta* (an eccentric plant in more than one respect) fully five feet in height, and widely branched. We do not apprehend that such statements will be discredited by any person familiar with the vegetation of such localities. We mention them as curiosities in vegetable growth, and not as matters worthy of permanent record, or of a place in a work of the nature of the "Manual."—*American Naturalist*, August.

An Intoxicating Fungus. The Fly Agaric (*Agaricus muscarius*).—It would seem, from a paper published by Dr. A. Kellog, that this fungus is more extensively used than we are aware of. The desired effect comes on from one to two hours after taking the fungus. Giddiness and drunkenness follow in the same manner as from wine or spirituous liquors; cheerfulness is first produced, the face becomes flushed, involuntary words and actions follow, and sometimes loss of consciousness. Some persons it renders remarkably active, proving highly stimulant to muscular exertion; but by too large a dose violent spasmodic effects are produced. So exciting is it to the nervous system of many that its effects are very ludicrous; a talkative person cannot keep silence or secrets; one fond of music is perpetually singing; and if a person under its influence wishes to step over a straw or stick, he takes a stride or jump sufficient to clear the trunk of a tree. It is needless to say delirium, coma, and death often result, as in the case of alcoholic spirits. The most remarkable fact is that the fluids of the debauchee become singularly narcotic, and are therefore preserved in times of scarcity. Thus a whole village, as some say, may be intoxicated through the medium of one man, and thus one fungus serves to prolong these most fearful and disgusting orgies for many days together. It is worthy of note that the very same erroneous impression as to size and distance produced by this plant, are also created by the *hasheesh* of India, and are also frequently noticed among idiots and lunatics.

The Circulation of the Culex.—Mr. H. C. Perkins gives an interesting paper in the *Monthly Microscopical Journal* for September. He says that, while watching the circulation as seen through the lenses in the reflected sunlight, if he moves the diaphragm from left to right, so as to make the shadow enter upon the right of the field of view, a brisk circulation (no matter how quiet it had been before) is instantly witnessed, which appears to be changed in direction as the diaphragm is moved back again; and that the direction of the circulation can thus be changed at will by the intercep-

tion of the sunlight. This same result can also be witnessed by the passage of clouds between the sun and mirror. The actual direction in the plant is from the apex of the leaf in sunlight and towards it in the shade. This change in direction is so rapid when produced by the shadow of fast flitting clouds across the sun's disc that it would seem that the change of temperature could hardly be felt by the plant; it certainly could not be by an ordinary thermometer; but a heated body properly placed will quicken the circulation, as cold will retard it. If he mistakes not, we have here a fine demonstration of the conversion of light into heat by its passage through the vegetable tissues, and of heat into motion by its action upon the laticiferous vessels.

CHEMISTRY.

How to Test easily the Fatty Oils.—In a paper published in the *Journal de Pharmacie* for July, Dr. Massie gives a method by which we may readily test the relative value. In order to test the purity of the oils, the author employs nitric acid (sp. gr. of from 1·38 to 1·41) and metallic mercury. Five grms. of this acid, and 10 grms. of the oil to be tested, are mixed together in a test-glass of 100 c. c. capacity, and the mixture stirred for a couple of minutes. The liquid is then left standing; and, when the fluid has separated into two layers, the colour of these layers is noted. This coloration may be from greenish-white to deep brown for the superior, or lighter layer; while the coloration of the acid, always less intense, will vary from light yellow to deep yellow, or even rose-red. After a while 1 grm. of metallic mercury is added, the final result of which addition will be the solidification of the bulk of the oil in most cases.

Alkalinity of Carbonate of Lime.—Mr. W. Skey, who we observe constantly sends communications to the *Chemical News*, and who is analyst to the Geological Survey of New Zealand, sends an interesting note relative to the alkalinity of carbonate of lime. He gives the results, which seem to show that the salt is alkaline rather than neutral. The following are results of several experiments. 1. Carbonate of lime, prepared by igniting pure oxalate of lime in a close crucible, at a dull red-heat, gives an intense alkaline reaction with reddened litmus-paper, after moistening with distilled water, or after re-ignition with pure carbonate of ammonia. 2. Carbonate of lime, prepared directly from chloride of calcium and bicarbonate of soda, by admixture of their aqueous solutions, and washing the ensuing precipitate till all the soda was removed, gave the same reaction with test-paper. 3. Limestone, shells (calcareous), calc-spar crystals, and arragonite, are all strongly alkaline to test-paper (at least, the samples he had tried were); the powder of any of these substances, washed with distilled water for many days, does not seem to lose any of this alkalinity. Lastly (and he thinks conclusively), precipitated carbonate of lime, prepared by either of the above processes, when agitated with weak hydrochloric acid, in successive quantities, until gradually reduced to a minute proportion of its original bulk, still manifests this reaction to an eminent degree; indeed,

the solution could not be rendered permanently acid till the whole of the carbonate was dissolved.—*Chemical News.*

Method of Treating Native Sulphides, &c.—The *Moniteur scientifique* for August contains a paper on this subject, by Dr. E. Kopp. He first alludes to the mineral resources of Italy, which appear to be far greater and of more value than is generally known or suspected. The difficulty of rendering these treasures industrially available is the great scarcity of fuel. Under these conditions, a series of experiments have been made, to ascertain whether it might be possible to apply cheap and readily-accessible chemical re-agents to act upon the above-named minerals (without simultaneously affecting the gangue), so as thereby to render them in a state fit for being readily converted into metals. As reagents available for the purposes alluded to, the author has found common salt, chloride of iron, and hydrochloric acid readily to suit the requirements. Among the practical suggestions found in this paper, is the fact, that the most economical method to extract the small quantity of copper present in previously-burnt pyrites, consists in first exposing the burnt substance for a time to air and moisture, and then to pour over the material a solution of common salt. A small addition of hydrochloric acid is very useful; the copper thus becomes converted into a soluble chloride.

Hydrogenium Amalgam.—A great deal of interest attaches to all attempts with this substance. We therefore give the following account of Dr. Loew's attempts as set forth in the *Journal für prakt. Chemie* (Nos. 6 and 7). He prepares the hydrogenium amalgam by shaking together, in a vessel to be kept very cool, a mixture of mercury containing from 1 to 2 per cent. of metallic zinc, along with an equal bulk of a solution of chloride of platinum containing 10 per cent. of solid chloride. A slimy mass is obtained, devoid of metallic lustre and prone to decomposition, owing to the presence of some zinc and some compounds of that metal; but, on treating the mass with dilute hydrochloric acid, a body having the consistence of butter is obtained, which, according to the author, is a true amalgam of mercury and hydrogenium.

Calorific Value of certain Gases.—In a paper read before the American Association at Salem, by Professors B. Silliman and H. Wurtz, there are some conclusions which will be of interest to our readers. From the second table it is clear—1. That, of all known gases, the highest calorific effects, under ordinary atmospheric conditions, are obtainable from carbonic oxide, whose calorific value, above 100° C., is about 3,000° C. 2. That, in absolute calorific value, below 100° C., in the atmospheric medium, hydrogen surpasses the volume of any other gas, giving a temperature of about 3,200° C. 3. That for all modes of application—that is, for producing both high and low temperatures—the total maximum calorific effectiveness of carbonic oxide is a constant quantity. 4. Compound condensed submultiple volumes of hydrogen, like that in marsh gas, have much less total calorific value in air than their volume of free hydrogen. 5. Condensed compound submultiple volumes of gaseous carbon, like that in olefiant gas, have no greater total calorific value in air, below 100° C., than their own volume of carbon gas in the form of carbonic oxide; while above 100° C. their value is even considerably less.

Mica a Substitute for Bronze.—The practical value of mica may by this means be much increased, but we doubt the possibility of making it a real substitute for bronze. The following quotation is taken by the *Chemical News* from the *Journal für prakt. Chemie.* The mica is reduced to small pieces in a stamping mill, digested with chlorhydric acid, cleansed by washing and sorted by sieves into sizes. The mica scales so prepared have a beautiful vitreous lustre, a silvery appearance, and bear in commerce the name of brocade crystal colours or mica bronzes. The advantages of these brocades over the common metallic ones are—1. They contain no unwholesome substance. 2. They possess a metallic lustre like the metallic brocades, and much surpass them in splendour. 3. Brown, blue, black, green, and red colours of rare brilliancy can be obtained, which is not the case with the metallic brocades. 4. They are not dimmed by sulphur vapours. The analyses by Drs. Cech and Schneider show that the colouring matter of the rose-brocade is cochineal; that of carmine is fuchsine; bright red, fuchsine, and Havanna brown; violet, Hofmann's violet; bright blue, Berlin blue; dark blue, probably impure aniline blue or Girard's violet; light and dark green, a mixture of aniline blue and curcuma; gold, curcuma; silver, the mica alone, &c.

How to Estimate Glacial Acetic Acid Quantitatively.—The *Chemical News*, quoting from a German journal, says, that F. Rüdorff preliminarily refers to the usual methods of the estimation in question, by means of a titrated soda solution, stating it to be unsatisfactory; and next mentions that his method is based upon the estimation of the freezing-point of the acid in question. The author enters at length into the details of his experiments, the chief point of interest of which is that perfectly pure and anhydrous acetic acid solidifies at 16°·7; that if either water, alcohol, some salts, or sulphuric acid are present, these substances tend to lower the point of solidification, so that 100 parts of the pure acid, mixed with 24 of water, solidifies at -7°·4.

Amorphous Silica for Fixing Dyes.—In Dingler's *Polytechnisches Journal* (second number for June) Dr. M. Reimann describes a series of experiments made with the view to apply amorphous silica (as obtained by precipitating a solution of so-called water-glass, silicate of soda, or potassa, with an acid, and collecting, washing, and drying the precipitate in the ordinary way) for absorbing the solutions of fuchsine, aniline blue, &c., and to apply the coloured powder so prepared as a pigment for various materials. The author states that glass, first superficially acted upon by hydrofluoric acid, and next mordanted, as is usual for cotton, assumes, when submitted to the processes in use for dyeing fibre, precisely similar colours as that fibre, and that this effect is caused by the amorphous silica contained in the glass and made active by the hydrofluoric acid.

A Test for Water in Milk.—It is, as is well known, a remarkably difficult matter to detect water in milk, so as to say for certain that it has been added. A test which appears likely has been devised by Dr. A. E. Davies, F.C.S. Such a test, he believes, we have in the specific gravity of the serum, or liquid portion of the milk, from which the caseine and fat have been removed by coagulating and straining. The gravity of this liquid he has found to be remarkably constant, ranging, in that obtained from genuine milk, from 1·026 to 1·028; and, by carefully ascertaining the specific gravity

of the serum of genuine milk diluted with various quantities of water, we may obtain a standard of comparison which will enable us to say, within a few per cents., what quantity of water has been added to any sample of milk that may come under our notice.—*Chemical News*, August 5.

A Simple Experiment in Reduction and Oxidation.—The following experiment has been described in the *Berichte der Deutschen Chemischen Gesellschaft zu Berlin*, No. 12.—A well-polished small copper bell is placed in a ring on a triangle, and made red-hot by causing a strong gas flame to play upon it, so as to render the metal red-hot, and, consequently, very soon black. As soon as this is the case, a strong current of hydrogen gas is directed upon the metal, by means of a flexible tube fastened to the neck of a glass funnel large enough to cover the bell. As soon as the hydrogen comes into contact with the red-hot metal, the layer of black oxide of copper is removed, and the metal appears as before it was heated. By removing the current of hydrogen, the oxygen of the air again acts upon the hot metal; and thus this alternate oxidation and reduction may be continued, provided the metal had been made thoroughly red-hot to begin with. The hydrogen should be pure, and free from traces even, of sulphur or arsenic, in order that the experiment be successful.

Reduction of Isatine to Indigo-Blue.—The *Chemical News*, in its admirable summary of the results of research abroad, gives the following note by Herren A. Bayer and A. Emmerling. They state that when isatine, previously pulverised, is mixed with fifty times its weight of a mixture of equal parts of terchloride of phosphorus and chloride of acetyl, to which some phosphorus is added, and this mixture is heated for several hours to from 75° to 80° in a sealed tube, and the bright green coloured liquid contained in the tube poured into a large bulk of water, next filtered, and then left standing for twenty-four hours in a large basin, a dark blue pulverulent substance is gradually deposited, which, being collected on a filter and washed with alcohol, yields a body in all its properties fully identical with indigo-blue. The quantity so obtained varies from 10 to 20 per cent. of the weight of the isatine employed. The authors enter at great length into collateral matters of interest as regards the synthesis of indigo-blue; but unless we were to produce a series of complicate formulæ, we could not give any useful abstract of that portion of their paper.

Researches on Alizarine.—A paper on this subject, which is now becoming popular enough among chemists, appears from V. Wartha in the *Berichte der Deutschen Chemischen Gesellschaft zu Berlin*, No. 12. The author states that, being engaged with researches on Turkey-red dyeing, he has found that the peculiarly brilliant red colour known by this term is due to a peculiar combination of alizarine with a fatty acid, which compound is soluble in a mixture of ligroine and ether, and does not even adhere very strongly to the cloth, since it may be readily removed therefrom by the liquid mixture alluded to. On evaporating this solution, there is left a brilliantly scarlet-coloured fatty substance, which, only after having been fused with caustic potassa, exhibits the characteristic reactions of alizarine. The author also states that the preparation of alizarine from madder is readily performed by first exhausting the substance (madder) with ligroine (light petroleum oil), and next treating it with a mixture of alcohol and hydrochloric acid (alcohol

saturated with the gas). On diluting this solution with much water, the alizarine is precipitated, in almost a chemically pure state, in the shape of an orange-coloured flocculent body. A carefully-conducted comparative investigation has proved to the author that the vegetable alizarine sublimes at between 130° and 140°. The synthetically-prepared artificial alizarine requires a temperature of from 280° to 300°.

The Formation of Ozone during Combustion.—In a recent number of the *Chemical News* it was stated by Mr. Loew that in a peculiar experiment ozone was formed. In reference to this Herr J. D. Boeke has recently been performing some experiments of interest. In Herr Boeke's experiment a stream of oxygen instead of air was blown through the luminous flame of a Bunsen's burner into the mouth of a glass balloon, and he really found that the air in the balloon had assumed a peculiar odour, and the property of colouring blue a mixture of starch paste and kalium iodide. Both changes are the result of the formation of a compound of oxygen and nitrogen (probably dinitric trioxide or nitric dioxide), not from the formation of ozone, as Mr. Loew asserts. The gas in the balloon being shaken with a little water, this was unable to colour the kalium iodide starch; kalium hydroxide, however, shaken with the gas, caused a dark blue coloration in the mixture, after having been acidified with dilute sulphuric acid. It is almost unnecessary to add that he had first assured himself by a blank experiment that the iodide was sufficiently free from iodate not to cause errors. With ferrous sulphate and strong sulphuric acid it gave the characteristic reaction of nitrates and nitrites. So when Mr. Loew declares that he was able to "identify the formation of ozone by its intense odour and the common tests," he was a little rash in this conclusion.

The Carbonates of Ammonia.—In a long and valuable memoir on the combinations of carbonic anhydride with ammonia and water, Dr. Divers has greatly extended our knowledge of these oft-examined yet imperfectly-described carbonates. From this memoir it appears that there are three, if not four, ammonium-carbonates which crystallise from their solutions, and that these have a simple, serial relation to each other. They are the hyper-acid carbonate (?), the acid or bicarbonate, the half-acid carbonate discovered by Rose, which the author shows has a formula different from that hitherto ascribed to it, and the normal carbonate, which the author considers Dalton obtained, but which will owe its recognition henceforward by chemists to the labours of Dr. Divers. By digestion at a gentle heat with strong solution of ammonia the carbonates of ammonium are converted into carbamate of ammonium, thus furnishing a very instructive instance of the dehydration of the ammonium salt of one of the simplest acids, and this, too, in the presence of water. The carbonate of ammonia, in commerce, is very uniform in composition nowadays, and this differs from the composition hitherto given to it. Instead of being $(CO_2)_3(OH_2)_2(NH_3)_4$, it is $(CO_2)_2OH_2(NH_3)_3$. When sal-ammoniac and chalk are heated together the reaction is not such as it is represented to be, and the product is not the carbonate of commerce. This is a result of the refining process; the products of the first distillation being ammonium-carbamate and water. These are some of the facts contained in this memoir; for others, and a mass of minor details, the reader should refer to the memoir itself in the June, July, and August numbers of the *Journal of the Chemical Society*.

GEOLOGY AND PALÆONTOLOGY.

Flint-Implements near Folkestone, Kent.—The Rev. J. M. Mello gives the following brief description of a splendid locality for flint-implements, which may be worthy of the attention of a few of our readers. He says that during the course of the last month, he found at Folkestone several flint-implements. Along the sides of the footpath on the top of the cliff between Folkestone and Sandown, there is a low embankment, made probably of material collected off the adjoining fields: in this embankment the implements occur. They are mostly of the rude flake or "scraper" pattern. The first discovered, which was also the finest, was lying partly exposed on the top of the bank, and subsequently his brother and himself found several more not far from the same spot. He has little doubt that further search in the same bank would bring many others, and possibly finer ones, to light.

The Coal at Korba.—In the May number of the *Records of the Geological Survey of India*, it is stated that the coal is exposed in two places in the bed of the Hasdo river, just below Korba. The thickness was estimated, though roughly, from its dip and length of outcrop, to be at least 90 feet, including bands of shale and inferior coal. In order to obtain a more correct idea of the quality of the coal, small pits were dug; these proved a minimum thickness of 50 feet of fair coal. Mr. W. T. Blandford, F.G.S., points out the best places for borings in order to ascertain the extent of the seam, as sufficient data are not known to justify the opening of a coal mine. Both the quality and mode of occurrence of the coal are considered favourable, and indeed, to surpass that near Chanda.

The Mammalian Fossils of Ireland.—A very able paper, but one of too great length for an abstract, has appeared on this subject in the *Geological Magazine* for September. It is by Robert H. Scott, M.A., F.R.S., and deals with the different historical statements of any value that have been made in reference to the distribution of mammalia in Ireland. In regard to the presence of the elephant and other fossils, a great deal of valuable information exists in the paper referred to.

A New Cephalaspis in America.—Mr. E. Ray Lankester describes an interesting specimen which was sent to him by Principal Dawson, of Montreal, Canada. The specimen presents in slight relief a small *Cephalaspis*, with head-shield and greater part of the body, and is much flattened. The shield appears to be larger in proportion to the body than in any British species. The orbits are not shown, and the matrix has not preserved the scales of the body with much distinctness, though it is possible to make out the lateral and marginal series. No trace of pectoral, dorsal, nor caudal fins is to be made out. This species clearly belongs to the section *Eu-cephalaspis* as defined in his monograph of *Cephalaspidæ*. Its best character as a species is to be found in the very fine, almost granular, tubercles which are preserved on some parts of the surface, and represent the apparently universally present tubercular ornament of the *Osteostraci*. These fine tubercles are more minute than on any British *Cephalaspid*, and, though seemingly not very well shown in this specimen, furnish a specific mark.

Dorypterus Hofmanni.—Mr. Albany Hancock communicated a paper,

through Professor Huxley, on this subject. The material for this paper consisted of four specimens of *Dorypterus Hofmanni*, which have been discovered by Joseph Duff, Esq., in the Marl-slate of Midderidge, and are believed to be the first examples of this fish which have been obtained in this country. The stratum from which they were procured is the same as that described by Prof. Sedgwick in the paper, published in the Transactions of this Society (2nd series, vol. iii. pp. 76, 77). The specimens show that the "ribbon-shaped" process mentioned by Germar is part of a peculiar exoskeleton, and that *Dorypterus* possessed ventral fins, which were situated in front of the pectorals, or "jugular." Hitherto, no fishes with ventral fins other than "abdominal" in position, have been known to occur earlier than the Cretaceous epoch. The tail is heterocercal, not homocercal, as Germar supposed. The dentition is not displayed in any of the specimens, and the teeth were probably small and inconspicuous; but the general structure of the fish shows it to be most nearly allied to the Pycnodonts.—*Geological Society*, June 22.

A Distal Portion of a Feather.—Prof. O. C. Marsh mentions that he has just received from Prof. F. V. Hayden the distal portion of a feather, with the shaft and vane preserved from a freshwater Tertiary deposit of the Green River Group, Wyoming Territory.

The Lias of the Banat Austria.—This, says Professor Rupert Jones in a recent lecture, has abundance of terrestrial plants, forming a coal; but here in the west the fossil trees and leaves of the Lias are but waifs and strays, and were washed to sea with the bones of the great *Scelidosaurus*; and the sudden river floods must have killed by the million successive generations of fishes, Ammonites, and Belemnites, and buried them in thick new mud, together with the unhurt carcases of the associated Ichthyosaur and Plesiosaur. These last have their skin and bowels intact; the molluscs were imbedded with the animal in the shell, and the cuttles retain even their inkbags unemptied, for death was quicker than their fear. Melting snow produces such sudden floods in temperate climates, and the monsoons on the eastern coast of India supply such abundance of fresh water, as to kill the sea-fish in myriads.

Mr. Poulett Scrope and Mr. David Forbes.—The admirable lecture delivered by Mr. David Forbes, F.R.S., before the Sunday Lecture Society, and which was published in the *Geological Magazine*, has undergone some valuable criticism by Mr. G. Poulett Scrope in the *Geological Magazine* for September. Those who go in for the views of either should read the opinions of both.

A New Species of Gavial.—Prof. O. C. Marsh, of Yale College, U. S., reports an interesting discovery. Some interesting reptilian remains have recently been obtained from the Eocene Greensand of Shark River, Monmouth County, New Jersey, indicative of a new species of Gavial, considerably smaller than any crocodilian heretofore discovered. They were found together, and are evidently parts of the same skeleton. They consist of various fragments of the skull, and ten vertebræ. The coössification of the neural arches of the vertebræ, and the almost entire obliteration of the sutures in some of them, would imply that the individual, although diminutive, was nearly or quite mature. The portions of the skull preserved indi-

cate that the animal had an elongated muzzle, and that the upper posterior parts of the skull were of the gavial type. The temporal apertures were large and near together. The teeth were not obtained. The vertebræ are well preserved, and present marked characters. The articular cup is transversely oval in the cervicals and anterior dorsals, and has its upper margin depressed in the posterior dorsals. The hypapophyses are simple and elongate. The neural canal of the cervical and anterior dorsal series is transverse and sub-rectangular in outline, and the floor unusually broad and flat. In the posterior dorsals, the canal, although still transverse, becomes less rectangular, with the broader portion above. The species may prove to be generically identical with the one named by the writer *Thecachampsa Squankensis*, which is the only crocodilian hitherto in the Eocene of New Jersey, but it is doubtful.—*American Journal of Science*, 2nd series, vol. i. No. 148.

A New Fossil Snake, Python Euboicus.—Professor Roemer, of Breslau, gives a description of a comparatively well preserved, and clearly identified ophidian, from the Tertiary limestone (Kalkschiefer) of Kumi, in the Island of Eubœa; which is so important as to merit the attention of palæontologists. The remains were found in a slab of limestone, 9 in. in length by 5 in. in breadth, which exhibits on its surface the vertebral column and ribs of an ophidian. It was procured for the University Museum from the cabinet of the late Dr. Beinert, and is stated to have been obtained from the wall of the Browncoal deposit of Kumi, and is probably of Miocene Tertiary Age, like those from the Braunkohlen formation of Germany. The portion of the skeleton preserved consists of part of the vertebral column, 9½ in. in length, and comprising 25 vertebræ with the ribs attached, also the greater part of the left ramus of the lower jaw, with eight of the teeth *in situ*. The specimen is so disposed upon the slab that about half the vertebræ exhibit their dorsal, and half their ventral, aspect.

A New Large Terebratula, which occurs in East Anglia, is described in the *Geological Magazine* for September, by Mr. E. Ray Lankester.

The Microscope in Geology.—Mr. S. Allport, F.G.S., has recently contributed a paper to the *Monthly Microscopical Journal* for August, which is of interest to all Geologists, as it shows them how useful is the microscope in their investigations. We commend the paper to the consideration of our readers. We give the following conclusions:—"Having now made upwards of four hundred sections of rocks and minerals, I am inclined to believe that the following results of microscopical examination will stand the test of further study. 1. The mineral constituents of the melaphyres and other fine-grained igneous rocks may be determined with certainty—a result which has not been attained by any other method of examination. 2. The mineral constituents of the true volcanic rocks, and those of the old melaphyres, are generally the same. 3. The old rocks have almost invariably undergone a considerable amount of alteration, and this change alone constitutes the difference now existing between them and the recent volcanic basalts. The basaltic lavas of the Rhine and Central France are composed of a triclinic felspar, augite, magnetite, olivine, and frequently apatite, the same minerals as those constituting the old rocks above described. I have fine-grained specimens of the latter hardly distinguishable from recent basalts; and a section of dolerite from the Puy de Barnère, in Auvergne, does not differ in

any important particular from coarse-grained specimens from Rowley. It would be easy to extend the parallelism to other classes of rocks, but I will now only observe that we have here another proof of the doctrine long taught by Lyell—the uniformity and continuity of the Laws of Nature."

Professor Morris's Testimonial.—This has been at last given to the Professor. A meeting was called on the 14th of July, at the apartments of the Geological Society, Somerset House, and a very complimentary, but by no means too much so, testimonial and 600*l.* were presented to the Professor. We trust the Royal Society will take the lesson.

A Fossil Hydrozoon, Palæocoryne.—The remarkable fossil on which Dr. P. M. Duncan, F.R.S. and Mr. H. M. Jenkins, F.G.S., have made their remarks in the *Philosophical Transactions*, 1869, was obtained from the lower shales of the Carboniferous Limestone series of Ayrshire and Lanarkshire, so rich in fossil Brachiopoda, Polyzoa, Crinoidea, and Madreporaria; and was found attached to the margins of the polyzoarium of *Fenestellæ*, and also in a detached and more or less fragmentary condition amongst the small pieces of broken Polyzoa and Crinoid stems which compose the fossiliferous layers of the shales. The base of *Palæocoryne* was expanded, giving rise to a short robust and cylindrical stem fluted and punctated on its surface, and surmounted by the body of the polypite from the upper margin of which radiate a single whorl of long and slender tentacles. On the upper surface of the body, a crateriform process, with an opening on its apex, indicates the position of the mouth. Its external investment appears to have been calcareous, covering the whole of the hydrozoon, except at the opening for the mouth and the terminations of the tentacles, which had probably ciliated ends projecting beyond the periderm or polypary. This is an almost solitary instance of a hydrozoon having a hard periderm, save the recent genus *Bimeria*, discovered on the west coast of Ireland by Dr. T. Strethill Wright. The Zoological position of the fossil is amongst the Hydrozoa in the order Tubularidæ, and near the Eudendridæ. Two species are described and figured by the authors, *Palæocoryne Scoticum* and *P. radiatum*.

MECHANICAL SCIENCE.

Bessemer's Steady Cabin.—Mr. Henry Bessemer has recently patented plans for the construction of cabins in sea-going passenger ships, which shall be perfectly steady, however much the ship may roll; he hopes that by this means sea-sickness may be prevented, and voyages undertaken in peace and tranquillity. Mr. Bessemer's plans, which have been worked out with the ingenuity and mechanical skill for which he is famous, only require a practical trial to prove whether or no he has really solved the difficulty, and placed it in our power to remove a serious barrier to intercourse between nations. The cabin in these plans is circular in plan, and is hung on gimbals at its centre, the point of suspension in the ship being so chosen that the cabin as a whole shall have as little vertical motion as possible. The mode of suspension secures that the floor of the cabin shall remain horizontal, but this is not enough. In so placing the cabin that the vertical motion is

practically abolished, Mr. Bessemer has made an advance in principle on all previous attempts in this direction. Mr. Bessemer is having a vessel constructed to test his plans.

Rifled Gun.—A gun now in course of construction at Woolwich is expected to prove the most powerful piece of ordnance ever produced. This is a 35-ton gun, carrying a projectile of 550 lbs. weight, propelled by a charge of 100 lbs. of powder. It is stated that this gun is expected to prove capable of penetrating a 15-inch plate, and judging from past experience it ought to be pretty nearly competent to accomplish so much. How ships are to be built capable of resisting such a projectile, if indeed that is possible, is yet to be seen.

Ventilation of Coal Mines.—Mechanical ventilation in coal mines is steadily gaining ground, on the older plan of producing a draught in the up-cast shaft by means of a furnace. Mr. D. P. Morrison recently read a paper on the subject before the North England Institute of Mining Engineers, at which he stated that in the deepest English coal mines, mechanical ventilation would show an economy of 35 to 40 per cent. over furnace ventilation. After discussing various arrangements of mechanical ventilators, he gave the preference to the Guibal centrifugal fan.

Steam Paviour.—In Paris, a steam paviour has been introduced to do the laborious work of the men with wooden rammers, whose appearance whereever a street is being relaid will be familiar to our readers. The French machine is similar in principle to a steam hammer, and is moved about when at work by a horse.

Ventilation in Railway Carriages.—Attempts are being made to secure more perfect ventilation in the carriages of the Metropolitan Railway, and to reduce the unpleasantly high temperature of the air. Experiments are being carried out on plans due to Dr. Croft, and are reported to have been successful, the anemometer showing a strong inflowing current, without any perceptible draught in the carriages.

New Ventilating Machine.—M. F. Mulhausen, a civil engineer of Brunswick, is said to have invented a new freezing and ventilating machine. The cold is produced by the expansion of previously compressed air, a process which in principle was suggested originally, we believe, by Prof. James Thomson.

Single Rail Tramway.—Mr. J. W. Addis, C.E., is experimenting in India on a new form of single rail tramway. The vehicles used in addition to the ordinary wheels have a pair of flanged wheels, one behind the other, running on the single rail, which is laid at the centre of the track. The flanged wheels are adjusted by a screw so as to take all the weight off the ordinary wheels, without lifting them much above the roadway. An experimental line has been laid, in part at an incline of 1 in 40, and along this a pair of bullocks draw a load of 3 tons. The advantages claimed for the system are—first, a very great diminution of power expended in hauling as compared with traction on common roads; secondly, that the cost of construction is only one-half that of an ordinary tramway with two lines of rails. A tramway or railway on a similar principle was, we believe, tried some time ago in France.

Rock Boring.—We learn from a letter in the *Engineer*, that the diamond

rock-boring machine of Messrs. Beaumont and Appleby recently drove a bore hole 84 feet, in thirty-six consecutive hours, through very hard rock, at a slate quarry near Portmadoc. A machine capable of accomplishing such a feat ought to prove of immense service both to mining and civil engineering.

The Captain.—While we write the news of the terrible catastrophe which has happened to this vessel reaches us. The *Captain* was the only vessel which completely exemplified the ideas which Captain Cowper Coles has so ably and so persistently urged on the Admiralty. As a mere fighting machine, she was one of the most powerful vessels afloat, if not the most powerful of any; and in her preliminary cruise she appeared to be thoroughly seaworthy. No particulars have reached us which would enable us to surmise whether she has fallen a victim to circumstances which no foresight could have provided against, or whether the catastrophe will prove that we have not yet completely mastered the problem of carrying enormously heavy armour on vessels of her class. In thinking over the peculiarities of the *Captain*, we cannot forget that her main characteristic, that to which her designer attached most importance, was an excessively low freeboard. Intended when first designed to have a freeboard of 8 feet only, and a height of port of 10 feet, she had actually, in consequence of alterations during construction, or for some other reason, a freeboard of only 6 feet and a height of port of 8 feet.—(*Engineering*, June 24.) She was therefore an extreme example of the low freeboard type of sailing vessel, for the American monitors, with a freeboard of only 16 ins., do not carry sails; and, in spite of the voyages of the *Monadnock* and *Miantonomoh*, have not yet established a position as ocean cruisers. The *Monarch*, designed by Mr. Reed as a sister vessel to the *Captain*, has a freeboard of 14 feet. In the trial cruise of the *Monarch* and *Captain* both vessels proved remarkably steady and easy in their pitching and rolling motions.

MEDICAL SCIENCE.

The First and Second Stages of Labour.—In regard to this point, a very able paper was contributed to the Royal Society by the Rev. Samuel Haughton, F.R.S. In the first stage of natural labour the involuntary muscles of the uterus contract upon the fluid contents of this organ, and possess sufficient force to dilate the mouth of the womb, and generally to rupture the membranes; and he endeavours to show, from the principles of muscular action already laid down, that the uterine muscles are sufficient, and not much more than sufficient, to complete the first stage of labour, and that they do not possess an amount of force adequate to rupture, in any case, the uterine wall itself. In the second stage of labour the irritation of the fœtal head upon the wall of the vagina provokes the reflex action of the voluntary abdominal muscles, which aid powerfully the uterine muscles to complete the second stage by expelling the fœtus. The amount of available additional force given out by the abdominal muscles admits of calculation, and will be found much greater than the force produced by the involuntary contractions of the womb itself.

On the Law which regulates the Relative Magnitude of the Areas of the Four Orifices of the Heart.—Dr. Herbert Davies, Senior Physician to the London Hospital, has published a very long and important paper on the above subject. It was read before the Royal Society many months since, and will require careful study. We quite think with the author, that there can be no doubt that an instrument so accurate in the adaptation of its valvular apparatus must reveal, on close examination, the existence of laws which not only determine the force required to be impressed on the blood traversing its chamber, but also the relative sizes of these apertures to one another.

What is Vaccinine?—The answer must be that it is a crystalline principle extracted from the leaves of the cowberry. The amount of vaccinine in the shrub is, according to Mr. E. Claussen, about 1 per cent.; it forms long acicular crystals, of somewhat bitter taste and devoid of smell. This substance is scarcely soluble in ether, better so in cold water and alcohol, but best of all in boiling water; a saturated solution of this substance in the latter yields, on cooling, a solid mass. When the crystalline substance is heated, it melts to a clear liquid. It is not precipitated by either sub-acetate of lead or tannin, is neutral to test-paper, and contains no nitrogen.—*American Journal of Pharmacy.*

The Action of Alcohol on the Body.—Dr. Parkes and Dr. Wollowicz have published in the *Proceedings of the Royal Society* a very valuable paper, from which we take the following:—It appears, then, clear that any quantity over two ounces of absolute alcohol daily would certainly do harm to this man (the subject of the experiment); but whether this, or even a smaller quantity, might not be hurtful if it were continued day after day, the experiments do not show. It is quite obvious that alcohol is not necessary for him; that is, that every function was perfectly performed without alcohol, and that even one ounce in twenty-four hours produced a decided effect on his heart, which was not necessary for his health, and perhaps, if the effect continued, would eventually lead to alterations in circulation, and to degeneration of tissues. It is not difficult to say what would be excess for him; but it is not easy to decide what would be moderation; it is only certain that it would be something under two fluid ounces of absolute alcohol in twenty-four hours. It will be seen that the general result of our experiments is to confirm the opinions held by physicians as to what must be the indications of alcohol both in health and disease. The effects on appetite and on circulation are the practical points to seize; and if we are correct in our inferences, the commencement of narcotism marks the point when both appetite and circulation will begin to be damaged. As to the metamorphosis of nitrogenous tissues or to animal heat, it seems improbable that alcohol in quantities that can be properly used in diet has any effect; it appears unlikely (in the face of the chemical results) that it can enable the body to perform more work on less food, though by quickening a failing heart it may enable work to be done which otherwise could not be so. It may then act like the spur in the side of a horse, eliciting force, though not supplying it.

The Poisonous Effects of the Icaja of Gaboon.—The *Comptes-rendus* of August 8 contains a very valuable paper on this subject, by MM. Rabuteau and Peyre. It seems that at the Gaboon there is in use a vegetable poison,

locally known as m'boundou, or icaja. That substance is the root of a plant, which is not further specified. The authors have been experimenting with this substance, which, even in very dilute decoctions, is very bitter, and appears to contain one or more alkaloids, since the aqueous decoction is largely precipitated by iodide of potassium, and also by phospho-molybdic acid. The poisonous effects of this substance bear some similarity to the effects of brucia, but the authors state that, under certain conditions, this poison does not hurt men. Some of the lower animals are readily killed by it; a dose of 3 milligrms. of the alcoholic extract, placed under the skin of a frog, kills it; and rabbits and dogs are killed by doses of from 15 to 25 centigrms. of the same extract introduced into the stomach.

Relation between White Blood Corpuscles and Pus-cells.—Very few of the many questions which have turned up of late years have received so much consideration as this one; yet it is still unsettled. If we may judge from a paper published by M. Picot in the *Comptes-rendus,* June 20, it would seem that the idea of Conheim, that the pus-corpuscles are partly produced by the passage of the white blood cells through the blood vessels, is altogether a mistake—is a misinterpretation of the phenomena in point. M. Picot, whose memoir was presented by M. Robin, gave a tolerably long account of his observations on the circulation of frogs and mammals, and he declares most positively that the white blood cells never pass through the vascular walls, and that the pus-cells are formed gradually, external to the capillaries. He explains the error of Conheim and others by stating that they confounded several focal planes together, and he considers that he has demonstrated this in the following way. He counted the number of white blood cells in the arrested blood in the capillaries, both before and after the quasi-exuded corpuscles appeared. In both instances, he says, the numbers were the same, and this could not have been if the white cells had passed outwards. We must, however, express our doubts as to the method by which M. Picot was so well able to count the corpuscles on both sides.

Relation of Pigment Cells to Capillaries.—The *Lancet* has called attention to some valuable researches of Dr. Saviotti, which we should be sorry to omit noticing. The observer was engaged in studying the inflammatory process in the foot of the frog, and he first obtained a circumscribed spot of inflammation by means of a drop of collodion, and after a few days found the pigment cells of the irritated spot accumulated around the vessels in a contracted condition, and in the course of a short time that they had entirely disappeared. He immediately applied himself to the question of explaining the mode of their disappearance. In other frogs he excited inflammation by dropping on the web a small quantity of a 2 per cent. solution of sulphuric acid. Again, after a few days, he saw that the pigment cells had accumulated around the blood-vessels, and that, though they still preserved their contractility, their processes were less branched and numerous than natural. On further examination, he now observed that these processes began to penetrate the walls of the adjacent capillaries and small veins, causing an obstruction to the onward movement of the red corpuscles on their proximal side, while a clear space was observable on their distal side, occupied only by serum. And now one of two things occurred: either the process of the cell broke off, and was swept away by the blood current, or

the whole cell gradually squeezed itself through the capillary wall (the part within the vessel becoming greatly attenuated and elongated) until it also was carried away. In the former case, the cell, shorn of part of its substance, still remained outside the vessel; in the latter, it of course disappeared entirely. As regards the time occupied in these phenomena, Dr. Saviotti finds that the cell processes penetrate the vessels in a period varying from three to six hours, and that it takes about the same length of time for the whole cell to follow and to be washed away from the internal surface, to which it long remains adherent.

Increase in Temperature of the Pulse.—Dr. Rattray has contributed a very valuable paper on this important subject to the *Proceedings of the Royal Society*, from which we take the following:—

	Temperate climate (near England), June, temp. 65° F., average of 10 days.	Tropics generally, average of 51 days.	Equator, temp. 84° F., average of 7 days.
9 A.M. . . .	98·1	98 51	98·5
3 P.M. . . .	98·3	99	99·5
9 P.M. . . .	98 5	98·47	99·1
Average . .	98 3	98·66	99·02

While observation thus showed that the average temperature of the body about the latitude of England is 91·3° F., the following Table shows that it rises in the tropics to 98½–99–99½, and occasionally even to 100° F. This fact is interesting, if not important, in connection with temperatures in disease; and the mutual relation of the two is worth study.

Hair Tonics, Washes, and Cosmetics.—It is of importance that the scientific world should not allow the immense trade in these articles to pass by altogether unnoticed. It would be neither well nor wise that it should be so. We are therefore glad to see that Professor C. F. Chandler has given some very interesting details in his Report to the New York Board of Health concerning the above. The following is a brief account of this gentleman's researches:—Hair tonics, washes, and restoratives; lotions for the skin; enamels; white powders for the skin. Of the substances named in the first category, sixteen were examined, all of which, with but one exception, were found to contain lead, generally in the form of the acetate or sugar of lead. No 11 among these samples is Mrs. S. A. Allen's "world's hair restorer," of 198 and 200 Greenwich Street, New York. One fluid ounce of this cosmetic (largely advertised) contains—Lead in solution, 5·26 grains; lead in sediment, 0·31 grain—total, 5·57 grains. The one sample free from lead was an ammoniacal solution of nitrate of silver, containing 4·78 grains of the nitrate in 1 fluid ounce. Of the lotions for the skin (six different samples were analysed), only one was made up with injurious metals, viz. an American compound known as "Perry's moth and freckle lotion," containing, to the fluid ounce—Corrosive sublimate, 3·61 grains; and crystallised sulphate of zinc, 4·25 grains. Among the enamels for the skin, seven different samples were tested, among which three containing from 108·94 to 190 99 grains of white lead in 1 fluid ounce; other

samples were found to contain oxide of zinc. The white powders for the skin were found to be chiefly made up of carbonate of lime and magnesia, clay, and French chalk, and as far, therefore, as these materials are concerned, are harmless, except in so far as their application may interfere with the healthy action of the skin.—*American Journal of Pharmacy*, July 1.

Physiological Effects of Carbonic Acid.—A valuable paper on this subject was read by Dr. B. W. Richardson, F.R.S., at the British Association meeting at Liverpool. The author first demonstrated from a specimen the result of subjecting a vegetable alkaline infusion to the action of carbonic acid under pressure. The result was a thick fluid substance, which resembled the fluid which exudes as germs from some trees. When this fluid was gently dried, it became a semi-solid substance, which yielded elastic fibres, and somewhat resembled conachone. (?) This observation had led the author to study the effect of carbonic acid on albumen, serum of blood, blood itself, bronchial secretion, and other organic fluids. When the serum of blood was thus treated with carbonic acid under pressure and gentle warmth (96° F.), the colloidal part was separated; but when the blood, with the fibrine removed from it, was treated, there was no direct separation, the blood corpuscles seeming for a time to engage the gas by condensation of it. But blood containing fibrine, and held fluid by tribasic phosphate of soda, was at once coagulated by the acid. The bronchial secretion is thickened by carbonic acid, and a tenacious fluid is obtained, resembling the secretion which occurs in asthma and bronchitis, while secretions on serous surfaces are thickened and rendered adhesive. After detail of many other facts, Dr. Richardson concluded by showing what bearing this subject had of a practical kind. In the first place, the research had relation to the question of elasticity of organic substances; and secondly, on the direct action of carbonic acid on the production of vegetable juices. But the greatest interest concentrated on the relation of the research to some of the diseases of the animal body. Thus, in instances where the temperature of the body is raised and the production of carbonic acid is excessive, the blood on the right side of the heart has its fibrine often precipitated, and in many other cases fibrinous or albuminous exuded fluids are solidified, as is the case in croup. The author, in the course of his paper, explained how rapidly blood charged with carbonic acid absorbed oxygen when exposed to that gas, and he held that carbonic acid in the venous blood was as essential to the process of respiration as was the oxygen in the pulmonary organs.

Methyl Compounds.—Among the many researches made during the year, and reported on to the British Association, Dr. Richardson said, among other things that had been discovered by the experiments made with anæsthetical bodies was, that it was possible to remove pain without removing consciousness, although any act performed by the patient was afterwards forgotten. The nervous centre which produced sensibility was affected and paralysed before those centres which were devoted to consciousness. He thought it very possible they would be able to discover an agency which would produce paralysis of sensation through the body without destroying consciousness at all.

Are the two Sides of the Brain alike?—Dr. Brown-Séquard thinks not. In the course of his remarks, at the British Association at Liverpool, he

said that the series of experiments he had made upon different animals led him to the belief that the right side of the brain was more important for organic life than the left side. Although the two sides of the brain were precisely alike when the animals were born, by greater development of the activities one side came to be quite different from the other.

Geological Systems and Endemic Diseases.—At the British Association at Liverpool, an important paper on the above subject was read by Dr. Moffat, showing that the soil has an influence on the composition of the cereal plants grown upon it, and on the diseases to which the inhabitants are subject. The district in which he practises consists geologically of the carboniferous and new red or Cheshire sandstone systems. The inhabitants of the first are engaged in mining and agricultural occupations, those of the latter in agriculture chiefly. Anæmia with goitre is a very prevalent disease amongst those living on the carboniferous system, whilst it is almost unknown amongst those residing on the new red sandstone, and consumption is also more prevalent amongst the inhabitants of the former. As anæmia is a condition in which there is a deficiency of the oxide of iron which the blood naturally contains, Dr. Moffat was led to make an examination of the relative composition of the wheat grown on soils of Cheshire sandstone, carboniferous limestone, millstone-grit, and a transition soil between Cheshire sandstone and the grit. The result of the analysis showed that the wheat grown on the soil of the Cheshire red sandstone contains the largest quantity of ash, and that there is a larger quantity of phosphates in it than in the soils of the carboniferous and millstone grit systems, and also a much larger quantity of oxide of iron than in either of them. He has calculated that each inhabitant on the Cheshire sandstone, if he consumes one pound of wheat daily, takes in nearly four grains more per day of the sesquioxide of iron than the inhabitant of the carboniferous system, who seems therefore to be subject to this great liability to anæmia in consequence of the deficiency of iron and phosphates in the food he consumes.

METALLURGY, MINERALOGY, AND MINING.

What is Glaucopyrite?—This is a new mineral, which our American cousins have introduced us to. The American *Engineering and Mining Journal* gives a full account, of which the following is portion. Speaking of the kidney-shaped masses, it says, they are entirely surrounded with lamellar calc-spar; and, if this is removed with acetic or dilute muriatic acid, the surface appears to be formed by numerous, mostly very small, crystals, in comb-like aggregations. On the layer of these crystals, Sandberger could discern, with a magnifying glass, as a fundamental type, twins of two flat rhombic tables, lying crosswise to each other, and presenting the habitus of cerussite—twins of similar combination; besides these, triplets can be distinctly recognised. The streak of the fine granular masses is shining, but the lustre of the crystal faces is feeble, except certain faces supposed to be normal prismatic and brachy diagonal prismatic, which have a strong metallic lustre. The colour of the mineral is light lead-grey to

tin-white, the streak greyish-black; hardness, 4·5; sp. gr. 7·181. In contact with the air, the mineral tarnishes slowly, first assuming a blackish, afterwards a yellowish-brown, and finally a blue colour. An analysis made by R. Senfter, in the laboratory of Dr. Petersen, gave the following result:—Sulphur, 2·36; arsenic, 60·90; antimony, 3·59; iron, 21·38; cobalt, 4·07; copper, 1·14: total 100·04. The new ore belongs, consequently, to the group of arsenical pyrites, and comes nearest to the mineral from Wolfach, which Sandberger identified with Breithaupt's geyerite. But, as neither form nor hardness, nor specific gravity and colour, are the same in both, and as, moreover, the copper observed in the new mineral is wanting in geyerite, Sandberger proposes, for this new ore from Guadalcanal, the name *glaucopyrite*, and gives for its composition the formula—

$$FeS_2 + 12\left.\begin{matrix} Fe \\ (Co) \\ (Co) \end{matrix}\right\} \begin{matrix} As_2 \\ (Si_2) \end{matrix}$$

The "Rochage" of Cast Iron.—An interesting letter on this subject appears from "T. B." in the *Chemical News* of August 5. It says that this curious phenomenon is well-known to those engaged at blast-furnaces. The sparks are known by the workmen as "jumpers," and their presence is usually held to indicate an approximation to white-iron. These sparks are absent during the running of grey-iron from the furnace, and only begin to make their appearance when the iron is about No. 4, the usual degree of greyness preferred in South Staffordshire for puddling. The sparks are best observed during the running of white-iron from the furnace, especially if the molten metal is not very fluid, at which times he has frequently observed a vast number produced, particularly in the channel; and sometimes, after the pigs have "set," little jets of sparks are continuously discharged for many minutes, which discharge is accompanied by a hissing sound. M. Caron's view may probably be correct, but he is inclined to attribute the production of these sparks to the combustion of carbon, and not of iron, as there is an entire absence of the peculiar scintillations displayed by burning iron. Yet one would almost be inclined to believe that grey-iron, which is supposed to contain uncombined carbon, would be more likely to exhibit this appearance than would white or mottled-iron. Yet, in grey-iron, and even in over-grey, or "Kishy" iron, there is an entire absence of these sparks.

Important to Paraffin-Oil Producers.—The Association for Mineral Oils at Halle, in the Saale, desires to obtain information relative to: 1. The best information concerning the chemical means of purifying crude paraffin cakes, so that the loss of paraffin does not thereby rise above 5 per cent. 2. A contrivance whereby materials containing paraffin may be cooled down at any season of the year to at least 5° C. In reference to No. 1, among other conditions are—that such substances as colourless tar oils, benzine, and, generally speaking, any substances which either dissolve or alter paraffin, are excluded from use; the operation, moreover, ought to be readily executed and suitable for working on the large scale: the paraffin should be free from smell, and be of a bluish-white colour. In reference to No. 2, the contrivance ought to be such as to enable to cool down daily at least 500 cwts. in quantities of 5 cwts. at a time. The Association is inclined to give two premiums, each at 750*l.* value, for the best of the contrivances asked for, the

time being fixed for January 1, 1871. Apply to the Mineralöl-Verein zu Halle a. d. S. Prussia. The committee of examination consists of several parties, among whom is M. A. Riebeck, at Halle.

Mineralogical Work.—The *American Journal of Science and Art* (July), gives a long account of several new mineralogical discoveries of Mr. C. U. Shepard, sen. They are summed up as follows: A new variety of columbite, found to consist, in 100 parts, of metallic acids, 78·30; protoxide of iron, 13 86; protoxide of manganese, 7·72; traces of tin. Unknown mineral (microlite?) in Haddam columbite. New locality of bismuthine and bismuthite, in Haddam. Metallic acid in microlite. Redondite, a mineral found in Redonda Island, and containing 44 07 per cent. of phosphoric acid, 24·73 per cent. of water, and oxide of iron and alumina. Phosphoric acid in the diaspore of Chester, Mass.; the mineral here alluded to is mainly hydrate of alumina, containing only 0·32 per cent. of phosphoric acid. The Pelham vermiculite contains, in 100 parts, alumina, 14·0; magnesia, 13·68; peroxide of iron, 32·0; silica, 24·0.

The Khettree Meteorite.—The *Chemical News* of June 17 contains a very long account, by Dr. Waldre, F.C.S., of this peculiar meteorite, which seems to have fallen very largely. The stone is partly of a light bluish-grey colour, partly of a much darker grey; in some places the two portions lying in contact like two strata, in others nodules of the one imbedded in the other. The broken surface is studded over with metallic particles, many of them having a bright metallic lustre; and there are also observable, by aid of a lens, spots of a yellowish or brown colour, from oxidation of the iron, and granules of a greenish-yellow colour and translucent appearance, probably olivine. Spherules of earthy matter are also visible, and round cavities in which others have been imbedded. When coarsely powdered, the spherules are more visible; and, when more finely powdered and examined under water with the lens, the lighter portion of the stone exhibits a considerable quantity of a nearly white crystalline matter, the particles of which are tolerably uniform in size, mixed with small angular fragments of black, brownish, opaque, and greenish-yellow translucent minerals, and irregularly-shaped but rounded particles of iron. The dark grey portion exhibits the same appearances, but with a much larger proportion of dark-coloured earthy minerals. The particles of the iron, having resisted trituration, now appear much larger than the others.

The Action of Heat on Diamonds.—A very curious influence exerted by heat upon diamonds has been noticed at the works at St. Helen's, recently visited by the British Association. When a diamond is used to cut hot glass the diamond will only last for one day, and it assumes a milky appearance. The diamonds in constant use for cutting cold glass last about three months. Each diamond costs from 35*s.* to 45*s.*, and is about three times the size of an ordinary glazier's diamond. Hot glass is cut more readily than cold glass.

Improvements in Heavy Forgings.—Lieut.-Colonel Clay read a short paper on improved appliances for the production of heavy forgings at the Liverpool meeting of the British Association. Considerable difficulty had until recently been encountered in the economical forging of large solid masses of iron, which was an engineering desideratum of vast importance. The old

constructions of furnaces were most imperfect and unreliable, some of them requiring renewing once a fortnight. Mr. Siemens had introduced the principle of heating large forges by means of gas, which attained intense degrees of heat and saved at least 50 per cent. in fuel. Until, however, the author of the paper attempted the experiment at the Mersey Iron Works, Mr. Siemens's principle had not been applied to the heating of large solid masses of iron. His application proved completely successful. He had also introduced improved facilities for the handling of the hot masses of iron, and for affording more working space for the men; and he had introduced a hammer with a clear unfettered fall.

Rolling Axles.—Mr. Alfred Bowater submitted a paper descriptive of a new machine, existing in model, for the shaping of railway axles by rolling pressure. Whereas by the steam-hammering process an axle required half an hour in shaping, this rolling process would effect it in a superior manner in two minutes. The rolled axle was not only superior in quality, but was more uniform in size, and could be produced much more cheaply. The machine consisted of three rollers, which were regulated so as to gradually press closer together, thus reducing the diameter of the bar and extending its length until shaped to the size required. Axles of any length could be thus rolled, with collars at any part of the tyre. The rollers were geared to revolve all in the same direction, and their friction imparted motion to the axle. The rolling process would obviate those flaws in axles which occaonally caused appalling disasters on railways.

MICROSCOPY.

An Erecting Binocular Microscope.—An instrument of this kind, which promises to be the binocular of the future, has been devised by Mr. J. W. Stephenson, and described to the Microscopical Society. It would be impossible to give a satisfactory explanation of the instrument without a cut, so we must refer our readers to the *Monthly Microscopical Journal* for August for further details.

The Presidentship of the Quekett Club.—The club has done wisely and well this time in its selection of a president in choosing Dr. Beale. No one could be so well qualified for the task. Dr. Beale is, without any question, the first microscopist in England. It is greatly to be regretted that his engagement on the evening of which the Royal Microscopical Society meets prevented his accepting the presidentship of that institution.

Photographs of Nobert's Lines.—Dr. Woodward, of the United States' Army Medical Department, has just presented to the Royal Microscopical Society and to a few private friends, four admirable photographs of the above. The first represents the 1st, 2nd, 3rd, and 4th bands of the 19 band-plate, magnified 1,200 diameters by Powell and Lealand's $\frac{1}{16}$th immersion. The second represents the 8th, 9th, 10th, and 11th bands of the same plate, magnified 1,100 diameters with the same objective. The third displays the 13th, 14th, and 15th bands of the same plate, magnified 1,100 diameters

with the same objective; and the fourth contains two photos, one a large one representing the 19th band of the 19 plate-band, magnified 1,100 with Powell and Lealand's $\frac{1}{16}$th immersion, and the smaller an enlargement of part of the other to 2,750 diameters.

Synchæta under the Microscope.—Dr. Hudson, in a recent article on this subject, in the *Monthly Microscopical Journal*, says that when once *Synchæta* is placed between two plates of glass in a drop of water sufficiently small to keep it within the field and tolerably within the focal length even of an inch objective, its characteristic motions cease; it swims incessantly round and across its prison, and at such a rate that the eye gets weary of following it. By gently compressing it one good view can be obtained, but then it is always the same, or at all events can be only slightly altered by reversing the compressorium. In consequence of this no attempt has been hitherto made to describe the top of the head, or to delineate the front or side view. Its outline when swimming with either dorsal or ventral surface at right angles to the line of sight is that of a boy's kite; and exactly where the kite's tassels would be are two movable lobes fringed with a ring of powerful cilia, by means of which the creature performs its various antics in the water. Though the cause of ciliary action seems to be a mystery, yet the various ways in which the cilia of the lobes must work to produce *Synchæta's* motions admit, he thinks, of being explained.

The Anatomy of the Chiton.—In the *Archives néerlandaises* (t. iv.) M. W. Marshall gives an interesting account of the above. He describes the tests as composed of two parts, covered with an epidermis: the articulament and the segment. The former is composed of four layers; the deepest of these consists of calcareous prisms placed perpendicularly to the axis of the animal, and further it presents the several zones having different degrees of colour; the second layer is harder and thicker, and is composed simply of very fine granules, it is porcellanous, translucent, and of a bluish-white colour; the third layer is, like the first, composed of prisms placed at right angles to the axis, viewed in section they have the appearance of being finely striated; the upper layer is of a very peculiar nature. The author says that each *articulus buccalis* is seen to consist of ten peculiar triangular bodies whose points converge to form the point of the articulus. Each of these triangles in its turn appears to be formed of a number of needles, so arranged that they help to give the triangular shape to the body, and are themselves constituted like a feather of a shaft and minute crystalline barbs, and possibly barbules. M. Marshall gives a series of figures depicting these singular structures, and in fact enters into so many details as to render it impossible for us to give a just abstract of his views.

Microscopic Illumination. Mr. Wenham and Dr. Pigott.—The result of the present controversy is at present undecided. Whether Mr. Wenham or Dr. Pigott is right remains we think to be proved. Dr. Pigott is certainly gaining ground, but then the question may be asked, Is not this because Mr. Wenham is remaining quiet? We cannot venture to say on whose side right lies, but the following quotation from a paper by Mr. Wenham may be of interest. "It is scarcely to be expected," says Mr. Wenham, "that those who have not in some degree been practically familiar with the construction of object-glasses, can be fully aware of the value of the mercury

globule in originating combinations. To the optician it is as needful as the callipers and straight-edge to the engine-fitter—every glass is separately tested by it. Its familiar readings show whether the work is going on right or wrong; by the indications of inward or outward coma whether the oblique pencils are correct, and finally, the least chromatic or spherical error can be ascertained by its means. It may be 'well-known to mathematicians that these globules are not perfectly spherical' (and mathematicians will be correct in all things), but setting aside the fact that the more minute the particle the nearer it approaches to a true sphere, it happens that shape is not of the smallest consequence, or whether it is illuminated by oblique light, for it is not the globule but the absolute point of light reflected from it that is used. The diameter of a mercury globule for correcting the highest powers from a $\frac{1}{8}$ upwards is only the one five-thousandth part of an inch. Perhaps some one who thinks it may advance the subject, will be good enough to calculate the size of the image of a small lamp-flame set at 4 inches distance, reflected from the surface of a convex mirror of $\frac{1}{10,000}$ of an inch radius. Dr. Pigott, by converting the microscope object-glass into a species of telescope, and viewing distant and minute discs of light, professes by means of the 'Aplanatic Searcher' ! ! ! to have discovered spherical error in all our best glasses, to the existence of which everyone else has hitherto been blind. Doubtless a very imposing or 'striking' demonstration may be made out of this, but it is easy to demonstrate that by so doing we are setting at naught the very qualities and advantages of large angular pencils. The conjugate foci are now so far distant, that large angular aperture no longer exists. A difference in the adjusting collar that would produce an enormous amount of spherical aberration, when the object-glass is tried on the globule test, or in its legitimate use as a microscope lens, is scarcely perceptible in the telescope arrangement, and though a badly-corrected glass may not form an image, yet I have no hesitation in affirming that a lens may be made to give perfect definition under the latter condition, that will prove utterly worthless as an objective for the microscope."—*Monthly Microscopical Journal*, July.

PHYSICS.

The Construction of Thermopiles.—In a recent number of the *Transactions of the Royal Society* Lord Rosse gives a paper on the construction of thermopiles, which, though we cannot abstract at any very great length, is of considerable importance. Alluding to his experiments, Lord Rosse says that, although the above experiments are far less complete than he could have wished, they are sufficient to show that the sensibility of thermopiles may be considerably increased by diminution of the section of the bars composing them; whether they may be with advantage reduced to a greater extent than he has already done he cannot say, but he is inclined to think that they may. He has ascertained from Messrs. Elliott that the alloys used by them in the construction of thermopiles, at the time when he received his from them, were 32 parts of bismuth + 1 part of antimony, and $14\frac{1}{7}$ of bismuth + 1 part of tin. If allowance be made for the substitution

of the first of these two alloys for pure bismuth, the difference between Elliott's pile and the pairs II. & III. will be rather greater. The pile by Messrs. Elliott, if made of the same metals as he employed, would have been reduced in power from 1 to 0·9. The construction of thermo-couples, on the plan he has described, is comparatively easy. In about two hours he was able to make one, and in more experienced hands their construction would be still easier. An experiment was made with one of the piles to ascertain whether, when the heat was not directed centrally on the pile, much diminution of power would take place. There *was* less deviation, in consequence of the increase of the mean distance which the heat had to travel before it reached the soldering; but he believes that this defect might be remedied, probably without diminution of the power of the pile, by increasing the thickness of the face, and leaving the dimensions of the bars the same.

Improved Bichromate Battery.—In a letter written to the *Journal of the Franklin Institute*, Mr. W. Poole Levison, of Cambridge, Massachusetts, says that in the spring of 1869, while making use of a small bichromate of potash battery, he discovered that the addition of nitric acid to the mixture of potassic bichromate and sulphuric acid, contained in its porous cups, conferred upon it the virtue of *steadiness*, without involving the evolution of annoying fumes. For over two months, during last summer, he had in almost constant action a combination of twenty-three large Bunsen cells charged with dilute sulphuric acid and the triple mixture mentioned, and "set up" openly upon the floor of his room. Not only did he work about it with perfect comfort, but left choice brass instruments in its immediate neighbourhood with impunity. Its energy never fluctuated, but after remaining for some time steady, declined, precisely as if the electro-negative plates were bathed in nitric acid only. To a cooled mixture of potassic bichromate solution and sulphuric acid (perhaps preferably in atomic proportions) add *nitric* acid. The proportion of nitric acid may be greatly varied, as its office is merely to transfer oxygen.

Opening of the Kepler Monument.—The following very interesting account of this is taken from *Les Mondes* (July 14). On the 24th of last June, the very small Swabian town named Weildiestadt, with hardly 2,000 inhabitants, was the scene of a festive gathering for the purpose of unveiling the statue of the celebrated Kepler, who was born in an humble cottage yet existing, and now known as Kylerhaus. The statue of the celebrated astronomer, executed in bronze, represents him seated on an arm-chair; in his left hand, supported by a celestial globe, he holds a scroll, upon which an ellipse is delineated; in his right hand he holds a pair of opened compasses. At the four corners of the pedestal, upon which the statue is placed, are smaller statues, representing Michel Mastin, the Tübingen professor who taught Kepler mathematics, and Nicholas Copernicus, Tycho Brahe, and Jobst Byrg, who assisted Kepler in making astronomical instruments. On the centre of the pedestal is simply placed "Kepler;" the other sides of this portion of the monument are embellished with bas-relief representations of incidents of Kepler's life.

The Effect of Artificial Light on the Eyes.—The *Chemical News* takes the following interesting paper from *Les Mondes*. It is by M. V. Meunier. The

author states that the great difference between sun and artificial light is due to the fact that, of the light emitted from the former, about half the quantity of rays are luminous and calorific at the same time; but, as regards our artificial light, for ordinary oil (colza oil), the amount of non-luminous, yet calorific, rays is 90 per cent.; for white-hot platinum, 98 per cent.; alcohol flame, 99 per cent.; electric light, 80, and gas-light, 90 per cent.; while for petroleum and paraffin oils, the amount is 94 per cent. It is this large quantity of caloric rays in artificial light which causes the fatigue to the eyes; but this inconvenience may, according to the author, be almost entirely obviated by intercepting the thermic rays by glass, or, better yet, mica plates. The use of these renders the light soft and agreeable to the eyes.

The Exact Comparison of Measures of Length.—It would seem from the second publication so long after the first one, that this paper is considered of importance. Illustrated with several engravings it was originally written and published by Herr. F. J. Stamkart, in Dutch, as far back as the year 1839, but is here (*Chemical News*) reproduced in French at the suggestion of Professor F. Kaiser, partly in order to prove to M. Steinheil, of Munich, that the author (M. Stamkart) had, some thirty years ago, already invented what the German *savant* has lately described under the name of *fühlspiegel*, and chiefly because M. Stamkart's invention is of the highest importance just now for the purpose of aiding the exact reproduction of the standards of length (*étalon prototype*) of the mètre.

Oxygen in Petroleum Wells.—It is stated by M. Widemann, who is connected with the works of the New York Oxygen Gas Company, that the use of oxygen in renewing and increasing the flow of oil in petroleum wells has been so successful that a regular trade has sprung up in oxygen gas for this purpose. The gas is injected into the wells through tubes, and, mingling with the hydrocarbon vapours, forms an explosive mixture, which, when ignited, completely opens seams which have become clogged, and thus renews the flow.—*Scientific American.*

Temperature of Last Winter in Europe.—The recent experiments of Dr. Dove lead him to believe that abnormally low temperatures travel from East to West and abnormally high temperatures from West to East. He has proved this by various experiments during the past winter.

Loss of a Distinguished Physicist.—The town of Mulhouse has just lost a very celebrated citizen, brother of the celebrated manufacturer, M. J. Dollfus. The deceased had left the pursuits of industry to devote his time entirely to science, and especially to geology and mineralogy. He was one of the most expert explorers of Alpine glaciers, and his extensive researches in science associate his name with those of Agassiz, Des Desor, and Des Martius. The deceased was a man of great wealth, and was highly respected by all who were acquainted with him.

Further Researches on Cotton Respirators.—Dr. Jouglet has been experimenting on the use of cotton respirators, and states that, by their application, the disease known as miner's anæmia, and also the dangers of the effects of lead, copper, and mercury, to those who have to handle these metals, or work in vapours or dust thereof, may be prevented.—Vide *Comptes-rendus*, August.

Ice Machine for Brewers.—MM. L. Martin and Vindhaussen have given lengthy and full descriptions of two apparatus based upon the principle that, if the expansion of a gas (atmospheric air in this instance) is effected by mechanical means, absorption of heat—in other words, production of cold—takes place. The ice-making machine produces 100 kilos. of ice at a cost of about sixpence. The cooling apparatus is so arranged as to effect a drying of the malt and cool the wort at the same time.—*Revue hebdomadaire de Chimie.*

The Emission of Heat.—Herr G. Magnus continues his observations on this point. The last instalment of this lengthy memoir is divided into the following sections:—On reflection of heat; description of experiments; reflection from the surface of other substances than silver, glass, rock-salt, and sylvine; reflection under various angles; results.—*Ann. der Physik*, No. 4.

Galvanic Element with one Liquid.—A description of such an apparatus is given in the *Revue hebdomadaire de Chimie* (July 14). It is a galvanic cell composed of zinc and carbon placed in a fluid made up of 40 parts of water, 4·5 parts of bichromate of potassa, 9 parts of concentrated sulphuric acid, 4 parts of sulphate of soda, and 4 parts of the double sulphate of potassa and iron. This element produces a very regular current. The zinc need not be amalgamated, and no gas is evolved.

A Straight-needled Galvanometer.—A description, illustrated by woodcuts, appears in the *Revue hebdomadaire de Chimie* by M. Bourbouze. The object of the inventor is to render slight deviations visible to a large number of students simultaneously.

Underground Temperature.—From observations that have been carried on from 1864 to 1870 by MM. A. C. Becquerel and E. Becquerel, it seems that at 36 mètres below the surface the temperature is constantly 12·47°, and that at a depth of from 36 to 26 mètres a very slight difference only is observed. The paper contains a lengthened series of tabulated results.—*Comptes-rendus*, July 18.

Contribution to Terrestrial Magnetism.—We learn from the Proceedings of the Royal Society that a valuable paper on the above has been sent in by General Sir Edward Sabine, K.C.B., the President. It is accompanied by maps of the declination, inclination, and magnetic force, which have been drawn at the Hydrographic Office of the Admiralty under the superintendence of Captain Frederick John Evans, R.N., F.R.S. The paper consists in great measure of Tables, giving the observation of each of the three magnetic elements, with reductions in every case for the secular change between the date of the observation and that of the epoch (1842–5) for which the maps are constructed.

ZOOLOGY AND COMPARATIVE ANATOMY.

Eyes in the Mole.—The mole is blind. Not so the fœtal mole, which Mr. R. I. Lee, in a paper read before the Royal Society (April 28), proves: Mr. Lee's paper is not very fully published in the Proceedings; but such as it is, it contains much information. It concludes as follows:—It must necessarily happen that many interesting observations are made in the course of

an investigation like that which has been briefly described, and many minute details might have been added to this account; but it appeared to me to be desirable to limit the details, as far as possible, to those which were sufficient to establish the remarkable physiological fact that the mole, at the time of birth, is endowed with organs of vision of considerable perfection, while in mature age it is deprived of the means of sight in consequence of certain changes which take place in the base of the skull, terminating in the destruction of the most important structures on which the enjoyment of the sense of sight depends.

The Measurement of Teeth.—In a paper laid before the Royal Society in May last, Mr. Busk gave some very important advice relative to the method of giving the measurement of teeth. He says that the respective measurements, which may be taken with a pair of sharp-pointed caliper-compasses, having been pricked out upon the equidistant horizontal lines, the points showing the length and breadth of each tooth are connected by straight lines, and a sort of figure is thus obtained which, in nearly all cases, will be characteristic of the *genus* or *family*, and in many instances sufficient to determine the species also. In some cases, as for instance in *Canis* and *Viverra*, the odontograms are at first sight so nearly alike that recourse must be had to the pattern of the teeth in addition, as before alluded to. In order to render figures of this kind easily comparable *inter se*, it is necessary that they should be drawn upon some common scale for the distance between the horizontal lines. This is, of course, entirely arbitrary, all that is requisite being that it should not be too great nor too small.

The Growth of Shells.—The Proceedings of the Royal Society (June 16) contain a very interesting paper by Professor L. Macalister on this subject. He says that while engaged in arranging the large collection of shells in the Museum of the University of Dublin, he was led to mak measurements of univalve shells in order to see whether any deduction o zoological importance might be drawn from these valuable geometrical observations, and more especially to determine whether it might be possible to arrive at constant specific numerical parameters in these cases; and in all instances he has been surprised by finding that, in well-formed shells, the ratios of the successive whorls have been specifically constant. In making these measurements, the points to be determined are three, viz.:—1st, the ratio of elongation of the radius vector of the spiral; 2nd, the degree of linear expansion of the generating figure in the successive whorls; and 3rd, the degree of translation or slipping of the spiral on the central axis. The second of these we may call the discoidal coefficient, and the third the helicoidal coefficient.

The Lower Races of Man.—Sir John Lubbock delivered an interesting address on this subject at Liverpool. Among the extraordinary points were those concerning marriage. Thus, the idea of marriage does not in fact exist in the Sandwich Island system of relationship. Uncleships, auntships, cousinships, are ignored, and we have only grandparents, parents, brothers and sisters, children, and grandchildren. Here it is clear that the child is related to the group. It is not specially related either to its father or its mother, who stand in the same relation as more uncles and aunts, so that every child has several fathers and several mothers.

Use of the term Homology.—A valuable paper on this subject appears in the *Annals of Natural History* for August by Mr. St. George Mivart, F.R.S. It would be idle to abstract it, for the conclusions it embodies would occupy the whole of our zoological space. We may, however, direct special attention to it. The author objects to Mr. Lankester's proposal to do away with the term Homology, and he gives very good reasons for his belief, and offers fair tribute of praise to Professor Owen.

The Origin of the Tasmanians.—Geologically considered, Mr. Bonwick states that this people have now become almost extinct, an old woman being the only survivor of the race. They were related in manners and in general physique to the neighbouring Australians, but were allied by black skin and woolly hair to the distant Africans, while they were assimilated by resemblance of language, customs, and habits of thought, to many races scattered over vast areas. The author seeks to explain this relation by constructing an ideal southern continent, whence all the dark-coloured races surrounding the Indian Ocean, and extending into the Pacific and southern oceans, may have radiated. He regards the Tasmanian as probably older than the Australian. Dr. Hooker, whose authority had frequently been quoted in the paper, pointed out the similarity and differences that obtain between the floras of Australia, Tasmania, New Zealand, South Africa, &c. It has recently been found that the flora of the Howe Islands is very unlike that of Australia, although so near to the coast. He protested, however, against the inference that the line of migration followed by plants is necessarily the same as that pursued by the higher animals. The president alluded to the great difference between the Australian and Tasmanian, especially in the character of the hair; and he regarded it as physically impossible that the Tasmanian could have come from Australia. He suggested that an interrupted communication by a chain of islands may have extended from New Caledonia to Tasmania, similar to that which now connects New Caledonia with New Guinea; and that by this means a low negrito type may have spread eastward over this area.

Marine Mollusca of the Gulf of Suez.—A Report by Mr. M'Andrew, F.R.S., was read at the British Association by the Rev. S. Tristram. It was entitled, "On the Marine Mollusca of the Gulf of Suez, in February and March, 1869." The total number of species of Mollusca obtained by Mr. M'Andrew in the Gulf amounted to some 818, of which 619 had been identified or described, the remaining 199 being still undetermined. In the list of named species were 355 not previously recorded as inhabiting the Red Sea, and of these, 53, including 3 genera, were new to science. Most of the undetermined species also would probably prove to be new to science, and all of them additions to the Red Sea fauna. The extraordinary dissimilarity between the fauna of the Red Sea and of the Mediterranean, which had been frequently remarked, and which appeared to be confirmed by further researches on both sides of the isthmus, showed, the writer said, that a barrier between the seas must have existed from very remote time; and this was not inconsistent with Professor Issel's statement, that an examination of the geological conditions of the isthmus led to the conclusion that the two seas were united during the Eocine and Myocine periods.

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